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PREFACE

Of all the publications by the Annual Reviews, Inc., the *Annual Review of Psychology* has the longest time gap between the selection of its authors and the publication of their chapters. It has for some time been the feeling of the Editorial Committee that having this prolonged fallow period is advantageous. It makes possible long-range planning and, happily for the *Review*, keeps each author-elect from an immediate realization of the magnitude of the task he has, perhaps incautiously, undertaken. But, of course, the procedure has the disadvantage that policy changes voted by the Editorial Committee one year are not put into effect for several years. Thus, the effects of the Committee's search for a more adequate topical arrangement of the chapters, which was begun in 1957 and was continued at the Committee's meetings in the spring of 1958, will not become completely visible until the publication of Volume 12 in January of 1961. However, as was said in the immediately preceding Preface, the arrangement of Volume 11 will reflect at least a little of the Committee's more recent thinking. Unfortunately, the exact nature of the changes will be by no means obvious from a scrutiny of the Volume 11 chapters listed on page vii of this volume. But perhaps at this time it need only be said that the new master plan envisages fractionation of all but a few of the 16 to 18 areas which have customarily been treated, with certain chapter topics appearing yearly, others every other year, still others every third or fourth year, and a few only occasionally.

It will be noted that the present volume follows fairly closely the topical pattern of Volume 9, except that there are no chapters on Perception, Industrial Psychology, or Engineering Psychology. Individual Differences, and Problem Solving and Thinking repeat again as headings, and Motivation, which was last considered in Volume 3, once again receives space. Unfortunately, Professor George Drew of University College, London, has been prevented by illness from writing a chapter on Comparative Psychology. For a second time the *Annual Review of Psychology* was enabled, through the aid of a contract between the National Science Foundation and the Annual Reviews, Inc., to publish a chapter on the state of psychology in the U.S.S.R. And, as was true a year ago, Professor Alexander Mintz was prevailed upon to write in this area of his specialty. It is to be regretted that the contract terminates with this volume. But, perhaps, a somewhat similar arrangement can sometime be made for one or more of the later volumes.

Contrary to custom, last year's volume did not contain a list of the authors for the chapters of Volume 10. This omission resulted from the fact that no errata were received during the year for Volume 9 or earlier volumes and it had been the general practice of the Annual Reviews, Inc., to print the author lists for the succeeding years on the backs of the errata sheets of its publications. However, the management has responded favorably to the

desires of a number of protesting readers and is willing to allow the *Annual Review of Psychology* to publish from now on its lists of authors-to-be, whether or not earlier authors report errors in their handiwork.

On January 1, 1958, the term on the Editorial Committee to which Professor Norman Cameron was appointed was completed. It may be recalled that Dr. Cameron had felt it necessary to resign some time before and that Professor J. McV. Hunt had been appointed to serve for him. It is a pleasure to be able to report that Dr. Hunt was reappointed to the Committee for a second term and Dr. Quinn McNemar for another period as Associate Editor. By the time this volume is published (January of 1959), Dr. C. T. Morgan will have completed his term of office on the Committee. The length of Dr. Morgan's period of service as a Committee member has only been equalled once before in the history of the *Annual Review of Psychology* (by Dr. J. E. Anderson). The Committee will miss the help given so effectively by its senior member. During the past year Mrs. Adele Fumino and Miss Marian Hays shared the office of Editorial Assistant and Miss Hays also compiled the subject index. The Editorial Committee deeply appreciates their services. Attention is also called to the fact that it is the Editorial Committee of 1956 (listed on page ii) whose members should receive credit for the selection of the chapter authors of this volume.

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DEVELOPMENTAL PSYCHOLOGY^{1,2}

BY GEORGE G. THOMPSON³

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Developmental processes interact with learning to determine all behavioral change. In this broad and descriptive sense, development and learning are the basic domains of general psychology. Since the variables in these domains are functionally interdependent, the scientist is confronted with serious theoretical and methodological problems. The psychologist with a primary interest in developmental influences must carefully specify the concomitant effects of the learning variables. The learning, or acculturation, specialist faces the same problem of controlling for developmental effects.

In my opinion there has been a gross neglect of developmental variables in current behavior theory. The mounting evidence on the pervasive and dramatic effects of early experience on later development, and the equally important influence of developmental variables on learning capacities make it doubtful that any of our current conceptual models can be expanded to encompass the complex development-learning interactions. The living organism is a dynamic and developing system, variable in its functioning according to inherent genetic antecedents which interact with selected environmental antecedents. We now know that alteration in one part of this system can have widespread and enduring consequences. The research findings of the past year provide further support for these opinions, and constitute some progress toward an identification of the relevant variables.

THEORY AND RESEARCH TECHNIQUE

The concept of development was critically reviewed by a wide array of scholars in the volume introduced and edited by Harris (74). This stimulating document commemorated three decades of research and productive scholarship at the Institute of Child Welfare, University of Minnesota. Dr. John Anderson, under whose direction the Institute became one of our strongest centers for developmental study, contributed one of the most stimulating papers. His analysis of the scope and interrelatedness of developmental processes underscores the complexities of behavioral change. The serious student of developmental psychology will want to read and reread the papers in this volume. Although they are understandably uneven in focus on the central problem, they embody the essence of the developmental

¹ The survey of the literature pertaining to this review was completed in April, 1958.

² In this chapter the following abbreviations are used: TAT (Thematic Apperception Test); WISC (Wechsler Intelligence Scale for Children).

³ The author gratefully acknowledges the assistance of Louis Koenigsberg and John McKinney in the preparation of this review.

concept. This concept is vague at our present state of knowledge, but it is undeniably basic to an understanding of the orderly growth of behavior.

In a more restricted groping toward conceptual clarification, Davitz (46) attempts to show how some of the variables related to fear, defensive tendencies, and maladjustment among children can be inferred from currently available findings. He implies that it's time to push ahead with hypotheses derived from empirical studies of children rather than to continue to draw from psychoanalytic theory. "Notwithstanding the provocative value of inferences about childhood from adult behavior, increased understanding of maladjustment in children depends primarily upon further research focused on the behavior and experience of the child." What Davitz fails to recognize is that the empirical relationships upon which he draws are without what Floyd Allport calls "structure." Theory without kinematic principles and laws has serious explanatory limitations. The now antiquated paradigms of psychoanalysis are admittedly based on outmoded analogies to biology and physics of another day, yet they tend to be preferred to a complete disregard for unifying "structures." As Sir Isaac Newton wisely noted, theories are never displaced by railing or negative evidence but only by more adequate and parsimonious theories.

The British symposium on contributions of current theories to child development should be noted. Bowlby (21) reviewed the present and potential contributions of the ethnological approach; Hindley (83) appraised the advantages of associative learning theories; De Monchaux (47) discussed a limited portion of psychoanalytic theory; and Anthony (7) drew parallels between the theories of Piaget and Freud.

The problem of defining divisions of the life span was given some attention by English (59). He found little agreement among fellow members of the Division of Developmental Psychology of the American Psychological Association in assigning age ranges to popularly ascribed stages in development. He concludes that all of the present distinctions are arbitrary. Their arbitrariness would seem to stem from their lack of developmental meaning or usefulness. What appear to be needed are truly developmental stages with important psychological properties like the "primary socialization period" in puppies described by Scott (147). Such "stages" would not be isomorphically co-ordinated to chronological ages in different organisms, but would define identifiable developmental processes importantly related to behavior change.

The longitudinal method.—This method of studying developmental processes has ancient roots in the early infant biographies. It has undeniable advantages but is beset with many operational hazards. Kodlin & Thompson (104) have done us the excellent service of a careful analysis of the unique features of the longitudinal method. In a question-and-answer rubric they have provided a useful guide to seven different criteria which should be considered before embarking on an expensive investigation of the same subjects over a period of years. The analysis shows that under certain

sampling conditions the subjects can be selected in unbiased ways so that conventional statistics are appropriate. The hazards of longitudinal study are noted and documented by events that have taken place in some of the better-known investigations in this country. The conclusion is drawn that some questions can be answered more directly and economically by either the cross-sectional approach or by a variation of the traditional longitudinal methods.

One of the more difficult problems in longitudinal study is a proper assessment of cultural change. While the subjects are growing up, the society in which they are living also changes. The importance of cultural variation is interestingly illustrated in Templin's (166) use of a general information test designed some 26 years ago for use with kindergarten children. Opportunities to learn certain things have faded away and new ones have appeared. There are fewer differences among boys and girls of today in general information than there were 26 years ago, although the differences between children from different socioeconomic groups still obtain. In commenting on the fewer correct responses among the children of today, Templin aptly notes that this "... does not necessarily mean that they are less well informed. It probably indicates that the questions were better suited to the experiences of children in 1928."

It should be emphasized that every psychological study is longitudinal (or developmental) in the broad sense that it extends over some period of time, however brief. In the absence of a control group we cannot be assured that the experimental conditions imposed are relevant antecedents of the observed consequences. They may simply be correlates of unobserved developmental functions or everyday experiences occurring outside the laboratory. For example, Broadhurst (25) failed to confirm the reported decrease in emotionality following gentling, but did find that one of the criteria of emotionality (frequency of defecation) steadily decreased with increasing maturity. Studies of gentling (which procedures take time) without an appropriate control group would lead to erroneous antecedent-consequent relationships. Levitt (113), in a survey of 18 reports of follow-up studies of children having undergone psychotherapy, found that recovery from neurotic disorders is no greater among children so treated and children from comparable groups who received no therapy. He suggests a tongue-in-cheek attitude toward the benefits of child psychotherapy until well-planned research is conducted with appropriate control groups.

New scales for measuring developing patterns of behavior.—Harris (73) developed a scale of social attitudes designed to discriminate children with varying reputations for responsibility as viewed by their peers. Mean scores on this scale showed a positive trend with increasing age, and were highly correlated with other measures of personal-social adjustment. The test items related to social and societal obligations (e.g., "People can count on me to get things done, without checking on me.") were only modestly correlated (r of .40) with items related to self-feelings and personal attitudes

toward responsibility (e.g., "I do my chores the very best I know how."). The author speculates that self-attitudes and personal habits may be less subject to change.

McCandless & Marshall (122) have developed a promising new technique for measuring the 4-year-old child's preferences for playmates during indoor and outdoor play and during story-telling periods. During individual interviews they presented each child with photographs of all his preschool companions and encouraged three choices in response to "Who do you like to play with (outdoors)?" for each of the three play situations. The choices were weighted in terms of the order of selection. The stability coefficients of the sociometric scores ranged from .45 to .71 over 10 to 20 day intervals of time. The children's scores were positively related to teachers' judgments of children's friendships, with correlations ranging from .16 to .68. This new sociometric approach has been used constructively in studies reported elsewhere in this review. It should give impetus to investigations of social relations and relevant correlates among young children.

Sarason *et al.* (145) have constructed a new test anxiety scale appropriate for use in the second through the fifth grade of the elementary school. The test items were chosen according to the following criteria: unpleasantness, physiological concomitants, and conscious awareness. "Yes" or "No" responses are elicited to questions like "Do you worry a lot before you take a test?" When the test was administered to almost 1700 children it was found that the anxiety scores increased irregularly with age. Anxiety scores correlated around .20 with teachers' ratings and around -.25 with IQ. The final 30-item scale is available from the authors for research purposes.

For psychologists who are planning a trip to another country Dennis (51) offers a simple technique for the study of children's cultural orientations. From his study of American, Lebanese, and Sudanese children he concludes that "... children's conceptions of the functions of identical things vary from society to society." Responses to the following kinds of questions were categorized: "What is a *boy* for?" The functional definitions of concepts like rain, gold, birds, wood, and father were studied. He found that the American child sees the world more in terms of pleasure and less in terms of work and duty than the Lebanese child. The American child also produces a greater variety of responses.

DEVELOPMENT AND THE LEARNING PROCESS

There is undoubtedly a very complex interaction between learning and developmental processes. Hebb is one of the few general psychologists who has proposed a theoretical model for this interaction. The comparative psychologists have collected most of the recent research evidence bearing on the problem. Research with human subjects has advanced very little beyond the pioneer experimentation and theoretical conceptions of McGraw. This

is an area so vital to an adequate description of man's changing patterns of behavior that it deserves a vigorous renaissance.

In an excellent review of 51 reports, King (101) lists the following seven variables as being relevant to determining the effects of early experience upon the adult behavior of animals: age of the animal when the experience is given, age at the time of the test, duration or quantity of the experience, type or quality of the experience, type of performance task required of the adult animal, methods for testing persistence of the effects, and relation of the experience to the genetic background of the animal. "Until the effects of the seven variables discussed in this review are analyzed further, it is possible to accept only the general hypothesis that some early experiences affect later behavior." He wisely concludes that we cannot unravel the interactions until we can accurately predict the effects of isolated variables.

King (100) also contributed one of the most provocative research papers on the effects of early experience on later behavior. He studied the adult aggression of 288 male mice of different inbred strains who were provided variable social experiences at different periods after weaning. The criterion measure of aggression was the latency of the fighting response when the mice were initially exposed to each other. With respect to the "critical period" hypothesis, he concluded that the effects of early experience may be altered, reduced, or eliminated by subsequent experiences, that early experiences may be specific to a given type of adult response (sexual behavior not related to experimentally produced aggressiveness), and that there may be different effects on individuals of unlike genotypes. "These are specific stages in ontogeny during which certain capacities for behavior appear; the manner in which these capacities are utilized determines subsequent behavior; if these capacities are not utilized they tend to be inactivated." This is a succinct description of the development-learning interaction.

Social stimuli as reinforcing agents.—The problem of pinpointing the quantity and quality of a reinforcement is troublesome in all areas of psychology. It is especially vexing for those psychologists who work with children. One can never be sure if the children are being influenced by the earned token, gum or candy, or by the smile (or its absence) on the face of the experimenter, or by the novelty of the research setting. The presence or absence of an expected verbalization may be more influential on learning than material rewards. Some activities embody their own reinforcements.

Brackbill (22) has shown that social and body contacts with infants three-and-a-half to four-and-a-half months of age can serve as an effective type of reinforcement. Extinction of the smiling response was studied as a function of intermittent vs. regular reinforcement of this social type. The results showed that intermittent reinforcement prolonged the time required for extinction of the smiling response. Thus it appears that social learning is possible in the very young infant. Gewirtz & Baer (66) have shown that

depriving the preschool child of social stimulation for 20 min. increases the reinforcing value of verbal expressions of approval. They also demonstrated that children who most characteristically seek adult approval (presumably have a greater social need) are most affected by approval in the experimental situation.

Gewirtz, Baer & Roth (67), noting the similar effects on preschool children of social deprivation and "low availability" of an adult, have speculated that these social conditions may be points on a simple dimension of social reinforcers. They are of the opinion that this dimension may be logically similar to the deprivation and satiation operations controlling the primary appetitive drives. It would appear that the possibilities of manipulating children's behavior in the laboratory by differential social reinforcements are manifold.

Motivation and goal setting.—Mussen & Jones (132) conducted a most interesting study of the relative drives of early- and late-maturing adolescent boys. An analysis of observer's ratings on nine drives showed the late-maturing youths to have high drives for social acceptance and for aggression. There were no differences between the groups of such drives as achievement, recognition, autonomy, and succorance. The authors conclude that physical retardation may have adverse effects on personality, while acceleration may favor better social and psychological adjustment. They suggest that the late-maturer's high social drives may stem from feelings of insecurity and dependence which get transmitted into childish, attention-getting social techniques.

Witryol & Calkins (176) conceptualize "dares" and their acceptance as childish means of gaining greater social acceptability with peers. They found this type of motivation to shift with increasing age from the flaunting of physical safety rules to the challenging of social authorities, although dares involving dangerous automobile driving are ominously popular among twelfth grade pupils. Dares are regarded as marginal social values with both negative and positive incentive properties. Battle (10) found the interaction of teacher and pupil values to be directly related to level of pupil achievement. The author defined values as activities or conditions which are "cherished." Sears & Levin (148) investigated some of the motivating conditions which may underlie the preschool child's preferred goals (and values). Children of this age tended to prefer tasks which insured more frequent success. There was no consistent pattern of responding after failure to reach a previously set goal, this fact probably reflecting greater variability in response after failure and punishment.

Transposition studies.—What conditions favor generalization along the relative vs. the absolute properties of two or more stimulus objects? Stevenson & Langford (160) trained 80 preschool children (3 months to 3 years and 11 months of age) to choose the smaller of two blocks, then tested half of them for transposition effects immediately after training and half after a 24-hr. delay. They found that transposition was significantly greater after

the delay. The delay presumably reduces the young subject's ability to discriminate the stimulus materials on the basis of absolute magnitude. They question the adequacy of Spence's theory of discrimination learning as adopted by Kuenne and others, "... until more is known about the effects of such variables as time and inter-set stimulus differences on generalization gradients, it is impossible to know whether the theory is capable of providing a satisfactory account for the present results."

Rudel (144) also found that a lapse of time produced greater transposition effects in a group of children 58 to 73 months of age. Large differences between stimuli favored absolute responses, while small differences favored response in relative terms. Lapse of time seems to have the same effect on transposition tendencies as decreasing stimulus differences. Verbalization (as measured) did not ensure or preclude relative responses. "The response we elicit in tests of transposition depends on the extent to which differentiation has proceeded and on how and when we ask our experimental question." The author notes that an increase in differentiation permits a classification of the stimulus according to an ever-increasing number of attributes. It appears that much research is needed on the basic problems of scaling perceptual discriminations and determining generalization functions under different experimental conditions before much new light can be thrown on the complexities of transposition. Terrell & Kennedy (167) have also shown that the nature of the reward during the initial learning influences transposition effects. A reward of candy produced more transposition responses than praise, reproof, tokens (to be accumulated for a reward of candy), or a light flash (control group). Although more transposition was produced by an immediate reward of candy than by tokens leading to a bag of candy, the difference was not statistically significant. This finding led the authors to conclude that 4- and 5-year-old children can learn effectively under delayed reward conditions.

THE DEVELOPMENT OF SENSORIMOTOR, PERCEPTUAL, AND COGNITIVE FUNCTIONS

The sucking response.—The ontogeny of this response has a peculiar fascination for psychologists, perhaps because it is such a pronounced and easily observed event in earliest infancy, or perhaps because of other reasons that lie deep in our unawareness. It is an obvious food-procuring response, yet it seems to be much more than this. For example, James (90) has shown that newborn puppies make sucking responses independent of "hunger" or food intake. When the proper external stimuli are present newborn puppies show the sucking response whether bottle fed, dropper fed, or injected with milk to the point of satiation. James speculates, "It is probable that there is no mechanism from the stomach to control the food-taking response at this early stage." When are these neuromuscular associations established? Are they developmentally independent of varied experiences? The answers to these questions are needed before we can properly

assess the relative merits of the opposing psychoanalytic and reinforcement theories concerning the effects of differential nursing experiences.

On the basis of a penetrating analysis of 57 studies and reports on the sucking response, Ross, Fisher & King (143) draw a number of conclusions. Although the sucking response of human infants can be increased by reward training, the possibility of a primary internal drive still exists. The fullness of the stomach of well-nourished puppies has no apparent effect on sucking. The weight of available evidence indicates that sucking frustration produces increases in the amount of non-food-getting sucking movements in both human infants and puppies. There is no good evidence drawn from adequately controlled investigations that sucking experience correlates with any dimension of later behavior. There is a series of important problems in this area which are difficult to approach with human subjects. It will probably be many years before the last word is heard on the meaning and significance of the sucking response.

Developmental trends in sensory experience.—Evidence is now available that the child's sense of balance increases with age, as do so many other sensory functions. Cron & Pronko (42) studied some 500 children's abilities to walk a 12-ft. 2×4 plank elevated 2 in. above the ground. They found that this ability increased from the 4- to 6-year-old group to the 11- to 12-year-old group, then tended to level off, with some decline in the 12- to 15-year-old group. Girls were superior from 4 to 8 years, but inferior beyond this age to 15 years. Zeigler & Leibowitz (179) also found that visual size-constancy increases as a function of age. The results obtained with 7- to 9-year-old subjects agree most closely with the law of visual angle, while the results with 18- to 24-year-old subjects were close to the theoretical lines representing the law of size-constancy. Benton & Meneffee (14) obtained only slight and questionable support (r s approximating .20) for the hypothesis that degree of hand preference is related to ability to make right-left discriminations. They question the clinical meaningfulness of grouping these developmental skills under such concepts as "laterality" or "body schema." In a study of related interest Swanson (164) found that mentally defective children made more errors to homologous tactual stimulation than did normal children of the same mental age.

Melzack & Scott (124) present interesting evidence on the interrelatedness of early experience and sensory development for pain. They reared 10 dogs in isolation from 4 weeks until 8 months of age. The dogs were reared in two-compartment cages so constructed that they couldn't see out but could be moved from side to side (for cleaning the cage) without handling and with a minimum of social contact. The 12 litter-mates who served as controls were raised normally as pets in private homes and in the laboratory. The restricted dogs required significantly more electric shocks before they learned to make the proper avoidance responses. They were found to be strikingly different in capacity to perceive pain in nose-burning and pin-pricking tests. "It is concluded that early perceptual experiences determine,

in part at least, (a) the emergence of overt responses such as avoidance to noxious stimulation, and (b) the actual capacity to perceive pain normally." Since there are such wide individual differences in the human response to pain, one naturally wonders whether or not certain early experiences might also lower the individual's threshold for response to pain producing stimuli. Certainly some individuals seem to suffer greater pain than others.

Frank (61) presented an interesting discussion of the origins and functional significance of tactile communication. He outlined the development of tactile sensitivity and showed how tactile experiences probably acquire communication value in relation to other media of communication. He raised several interesting questions about the effects of deprivation of early tactile experiences, and suggested cultural differences in man's appreciation of tactile stimulation. Frank's analysis supports a very plausible interpretation of the relationships between tactile experiences and certain dimensions of personality development.

The acquisition of basic concepts.—The manner in which children acquire concepts and relate abstract symbols continues to be an area of active investigation. On the basis of his study of a set of twins and three singletons and their responses to mirror-reflected images, Dixon (54) concluded that self-recognition appears at about one year of age—an earlier age than previously reported by Gesell and Shirley. Burns & Cavey (30) showed that children from 3 to 5 years of age become more perceptually sensitive to facial detail of a subject in an incongruous situation (boy smiling as doctor holds a long hypodermic syringe), and less likely to project their own feelings to the gross aspects of the stimulus situations. Their interpretation of these age differences in terms of Piaget's theory of decreasing egocentricity is not very convincing. It should be recalled that adults also misinterpret situations in terms of their own perceptions when they fail to perceive minor features of a stimulus display. Labeling these responses "egocentric" would have doubtful explanatory meaning or predictive usefulness.

The development of conceptual interrelatedness.—The most substantial contribution in this area was a theoretical paper by Brown (27). He argued in a highly convincing fashion that children's verbal concepts are not necessarily acquired from the concrete to the abstract or from the abstract to the concrete but more typically in both directions from a middle level of abstraction. Adults name things so as to categorize them in a maximally useful way. The child first acquires those concepts most frequently heard in personally rewarding situations. For example, he may first learn "Rover" for a family pet and then move upward in abstractions to "dog," or he may first learn "money" and move downward toward the more concrete with "nickel," "dime," and "penny." "... the sequence in which words are acquired is not determined by the cognitive preferences of children so much as by the naming practices of adults." According to this hypothesis one can learn much about the child's acquisition of related concepts by studying the verbal practices in his culture.

Danziger (43) studied the child's growth in understanding of kinship terms over the 5- to 8-year period. Each child was asked such questions as: "What is a brother (sister, daughter, uncle, or cousin)?" "Have you got a brother?" "Are you a brother?" "Is every boy a brother?" and so on. Analysis showed that the younger children emphasized "categorical" answers (e.g., brother is "a boy," "a big boy," "a little boy"). With increasing age categorical answers decreased and "relational" concepts appeared (e.g., "a boy which is a relation to you," "a boy that belongs to another girl"). The kinship system with its definitional derivatives gradually makes its appearance. Weinstein (171) found that children between 5 and 12 years of age gradually acquire related concepts about their national identity and the properties of the flag (r of .76 between age and conceptual status). A Guttman type scaling indicated that the elements and their interrelatedness are organized in a fairly stable order from child to child. Bousfield, Esterson & Whitmarsch (20) noted an increase with age from third grade to college in total clustering in a sequence of recalled items. The developmental and learning experiences that promote conceptual interrelatedness deserve a great deal of study, since these functions appear so intimately associated with human intelligence.

Many variables influence conceptual meaning and growth. Nash (134) found that parts of the body tended to be identified as masculine or feminine according to their general size. Siegel (153) demonstrated that second-grade children's conceptions of social roles could be significantly influenced by differentially biased radio dramas. When a taxi driver was presented as resolving a conflict by physical aggression the children later viewed aggressiveness as closely associated with this social role. It has long been established that children of middle socioeconomic status are more precocious in verbal concept development than children of lower status. Siller (156) reaffirmed this finding but concluded that the relationship is considerably more complex than previously believed. His data suggest a significant difference in concept development between high school students whose lower-class parents are upwardly mobile and those who are not so oriented.

Piaget's notions about concept development continue to produce both positive and negative sentiments. Berlyne (15) reviewed his works in a sympathetic manner, but Klingberg (102) declared that his theories have little connection with modern science since they are based on "recapitulation theory" and "the doctrine of the primitive mentality of primitive man." Klingberg asserts that animistic tendencies are important socially and pedagogically but not theoretically. He studied the tendencies of 7- to 10-year-old children to attribute life to nonliving objects and found that these tendencies exist with children up to 10 years of age. However, he rejected the idea that this is caused by an "animistic tendency." Honkavaara (85) found that 5- to 8-year-old children are more likely to attribute life to objects toward which they have a positive affective tie. For example, a

"good toy" is more likely to be considered "alive" than an "old toy" or a "pretty dress" more "alive" than an "old dress." Dennis (49) found that 79 per cent of a sample of Near Eastern college and high school students gave one or more animistic responses to his question. This is much higher than the percentage found among American students. He attributes this finding to cultural differences in scientific concepts and information.

Hiram (89) demonstrated that children's logical thinking can be improved by giving them instruction in the basic concepts of logical thinking. Kendler *et al.* (99) conducted a clever and significant investigation of the variables related to the acquisition of inferential skills. They demonstrated that inferential behavior, like the simple bar-pressing habit in animals, is influenced by independent variations of reinforcement and motivational variables. Their research also emphasizes the hazards of believing that young children are being affected only by the experimentally presented variables. Some children displayed inferential behavior in the absence of the positive reinforcement or motivational conditions especially provided by the experimenter. They acknowledge the social complexities of reinforcement and motivational variables that lie outside the experimenter's control. "The S's motivation to please the adult E, to follow instructions, and the reinforcements derived from these motivations can be expected, for the present, to play a significant role in research of this sort." Every research person who anticipates working with young children in a laboratory situation will want to take cognizance of this observation.

DEVELOPMENT OF INTERESTS, ATTITUDES, AND ABILITIES

Interests and preferences.—There is now a hard core of research evidence showing that animals acquire some of their interests and preferences for commerce with environmental objects in response to developmental processes. Animals born and reared under the restricted controls of laboratory conditions evidence preferences for activities with which they have had no prior experience. They persist in approaching and avoiding situations in the absence of either positive or negative reinforcements. Thus it appears that the development of neurophysiological structure is correlated with some approach-withdrawal tendencies which are independent of previous experience.

An obvious assignment for developmental psychology is to identify those periods in the life span during which certain interests and preferences emerge. Once these developmental periods are identified it should be possible to investigate their susceptibility to environmental influence. The outstanding research on this problem to date has been the work of Beach on sexual behavior in lower animals. The research on human subjects has been primarily descriptive and normative.

Mischel (127) studied the preferences of 7-, 8-, and 9-year-olds for an immediate reward of a one-cent candy versus a ten-cent candy to be delivered

one week later. The results showed a steadily increasing preference for the larger but delayed reward with increasing age. This trend is attributed to learning experiences.

With increasing age, the potentiality for developing a strong expectancy of this kind [that the promised reinforcement will issue from the social agent in spite of time delay] increases if the individual continues to gain reinforcing experiences within this area, thus building up the relevant expectancies, but not as a function of growing older or biological maturation per se.

This is the type of study which begs the question of developmental influence.

In a study by Stewart (161) the process of making decisions in a preferential situation was investigated among nursery school and fourth-grade children. The subjects were first asked to rank 12 ten-cent toys. They were then asked to choose between pairs of most liked and of least liked toys. The toys were then ranked again after a lapse of 8 to 10 days. It was found that the nursery school children were less consistent in their choices on both ranking occasions. They were little influenced by the valence of the objects when asked to choose one of two toys. The choice behavior of the older children reflected a more stable and clearly defined system of valences. Most of the children found it more difficult to make choices between pairs of objects they disliked. The majority of these findings were predicted from Lewin's cognitive-field theory.

Collier & Gaier (39) attacked the problem of children's preferences by asking adults to recall their favorite childhood stories. College students were asked to give their past and present impressions of their selected stories. They found that women chose stories involving evil mother figures, benign but active males, and persecuted but passive young women. Their stories had happy, often magical endings. It was inferred that these stories stemmed from the Oedipal period. The men's preferred stories were filled with themes of adventure, problem-solving, and self-assertion. The story endings were generally reality-oriented. Women figures in the men's stories were almost exclusively kind and maternal. The male subjects reported that most of their preferred stories had been encountered in independent reading during the latency period. It was concluded that preferred stories reflect cultural, and possibly biological, sex-role expectancies. It would appear that they may also reflect a cultural residue of stereotyped literature, as suggested some years ago in Child's analysis of textbooks in reading.

In a carefully designed and executed study Rogers (141) used the paired-comparison technique to investigate the musical preferences of 635 pupils enrolled in the fourth, seventh, ninth, and twelfth grades of several public schools. He found a sharp decrease in children's preferences for classical music with increasing age. There was an overwhelming preference for popular music at all age levels. This report raises an interesting question.

Could musical preferences of this sort be changed by a one-sided exposure to classical music or are there developmental tendencies which favor the rhythm, simplicity, and repetitiveness of "popular" music?

Carsley (33) surveyed the interests of 10- and 11-year-old English children in reading books. The responses of approximately 2000 subjects indicated that films, television, and radio seemed to stimulate an interest in reading certain books. In school they preferred to have the teacher rather than other children read to them. They also preferred to read at home rather than in school. About half of them used the facilities of the public library. Blakely's study (18) shows that the reading of comic books is still a popular pastime with seventh-grade pupils. Of the children studied, about three-quarters of both boys and girls were regular readers. Jefferson (91) shows that parents are very perceptive about their children's preferences in reading material. They are sensitive to age and sex differences and the current interests of their children.

Attitudes and values.—We infer that attitudes and values are predispositions to behave in certain overt and covert ways. It has been shown that they have both energizing and directive functions. Since they become manifest with increasing age and are sensitive to environmental influence, it is commonly assumed that they are learned behavior tendencies. Individual differences in attitudes and values are generally attributed to different life experiences. Is this the complete story, or only the incomplete segment revealed by currently available research findings?

Muller (130), the distinguished geneticist at Indiana University, raises the provocative question of whether there may have been in man's history a "step-by-small-step development of the genetic bases of social feelings and behavior—that is, of the complex sometimes comprised under the term *brotherly love*." He shows how such behavior tendencies might have had survival value and have been transmitted genetically along family lines. His hypothesis brings genetic influences back into the picture as important variables in social response—a position much emphasized in current research in comparative psychology. The prospect must be faced that much of the variance among individuals in aggressiveness, dependency, nurturance, and so on may be due to differential genetic factors. Moreover, these genetic effects may have variable timing in different individuals. Muller's suppositions make man's hopes for a brighter new world through environmental control of experiences less tenable.

It has long been evident that man is not verbally aware of many of his important attitudes and values. Inferences about the covert behavior tendencies must be made on the basis of overt responses. The projective approach seems a promising technique for uncovering these unconscious behaviors. As part of a much larger investigation Harris & Tseng (75) presented four incomplete sentences to 3000 children from third through twelfth grades. Responses to the stems "My father . . .," "My mother . . .,"

"Most boys . . ." and "Most girls . . ." were analyzed into positive, negative, and neutral categories. It was found that the attitudes of boys and girls both were predominantly favorable toward parents and peers. The children's attitudes were more favorable toward members of their own sex, although girls expressed more negative attitudes toward their own sex with increasing age. The boy-girl antipathies of the intermediate grades are primarily the result of the girls' attitudes toward boys. The small proportion of children with negative attitudes decreases steadily among boys with age, but increases among girls. The mother is usually favored when differences exist in attitudes toward parents; however, girls show a more pronounced increase with age in positive attitudes toward fathers than toward mothers.

Mothers were also favored over fathers in a study of 730 fifth-grade children by Hawkes, Burchinal & Gardner (78). Boys reported less satisfactory relations with parents than girls. Only about one-fifth of the subjects indicated that they were "seldom" or "never" punished for disobeying their parents. Most of these children indicated that they were satisfied with their relationships with parents. These same children were employed to study the characteristics of the Hawkes-Lewis Scale of parental control (77). The test-retest stability of this scale over a one-week interval with 49 children was found to be .85 for both forms (one form based on adult judges' weights for items and one derived from the responses of a separate group of children). The total scores on this scale correlated between $-.40$ and $-.48$ with the family relations dimension of Rogers' test of personality adjustment. This scale was designed to measure an important area in children's attitudes. Further research is needed to ascertain its usefulness.

Are there unique attitudes and values peculiar to the adolescent youth? Hess & Goldblatt (82) found that adolescents and parents don't see eye-to-eye on an answer to this question. They solicited ratings from 32 adolescents and 54 parents on 20 bipolar adjectives representing aspects of character and personality within four frames of reference: average teenager, average adult, teenagers from adult's viewpoint, and adults from adult viewpoint. They found that adolescents idealize adults and believe that adults depreciate teenagers. Adolescents also see greater status differences between teenagers and adults than do parents. It is concluded that the adolescent views his problem in terms of ego functions (autonomy, self-control, and judgment), while parents see the teenager's problems in terms of the control of id impulses. Much of the protective bravado and complacency of the modern adolescent is traced to his idealization of the adult role and his feelings that his achievements are not recognized or appreciated. Livson & Nichols (115) also trace the male adolescent's social attitudes back to the influence of his subcultural milieu. An inverse cluster analysis of the E, F, and FEC items from the "Authoritarian Personality" questionnaire clearly demonstrated the effects of socioeconomic status but gave little support to the hypothesis that social attitudes are expressive of

"deep-lying" personality factors. Of incidental interest, Siegman (155) has shown that the antidemocratic attitudes of children 9 to 13 years of age are inversely related to age and intelligence, positively related to manifest anxiety, and curvilinearly related to strict observance of the Jewish religious faith.

Morris (129) has shown that adolescents' value judgments evidence greater autonomy and equity with increasing age, although there are still obvious discrepancies between what "should" and "will" be done. There was some indication of value fluctuation during the early adolescent years (called pubescent fluctuations). Hemming (81) made an interesting arm-chair analysis of the acquisition of moral values, or "internalized controls."

Language abilities.—Language, the uniquely human ability, is not an area of intense research interest in contemporary developmental psychology. The problems have not been solved, but the motivational impetus of Piaget's pronouncements some 20 years ago has largely spent itself. A bold new model is sorely needed. Perhaps the language mediation hypothesis in learning will provide the spark for a new type of developmental inquiry. However, this research contingent has not yet shown the way.

A purely descriptive study of growth in oral and written expression was conducted by Harrell (71) with 320 children from 9 through 15 years of age. The subjects were shown a movie and asked to write a story about it. Then they saw a different movie and were asked, individually, to tell a story about it. Analysis of these reports showed that both written and oral stories increased in length with age, that oral stories were longer than written, that repetitions and corrections decreased with age, and that girls wrote longer stories than boys (the only difference between the sexes). There was no indication for any of the measures that a mature level had been reached by 15 years of age in either oral or written stories.

Carrow (32) compared the linguistic functioning of monolingual and bilingual third-grade children. The monolingual children excelled significantly in oral reading accuracy (fewer articulatory errors), hearing vocabulary, oral reading comprehension, and speaking vocabulary but had no significant advantage in silent reading comprehension, spelling, silent or oral reading rate, or verbal output. Girls excelled boys in only one language function, oral rate of reading.

Wood (177) has favored us with an analysis of intellectual, sensory, and emotional factors often associated with delayed speech and language development. Blackman & Battin (17) added some substance to the framework with a case study of delayed language attributed to emotional disturbance. Johnson (95) reviewed 179 studies and reports on factors related to disability in reading, and Krantz (105) showed that development of reading ability specific to a content area is highly important to achievement in this area in elementary and secondary schooling.

Intellectual abilities.—The longitudinal study of intellectual growth con-

ducted at the Fels Research Institute was the outstanding study of the year. The findings were presented in abridged form in one professional journal (96) and in full detail in a monograph (157). The basic data included Stanford-Binet IQs and other longitudinal records on 140 Fels children collected at various times from infancy through ten years of age. The mean IQ of the group was approximately 120 at all age levels.

The principal purpose of the study was to find personality characteristics which might be correlated with upward or downward trends in IQ change. A smoothed curve was fitted to the several IQs available for each child. Then each subject's smoothed IQ at age 6 was subtracted from his smoothed IQ at age 10. This provided a distribution of positive and negative IQ changes. The median amount of change was found to be 17.9 IQ points. The greatest change was approximately 58 smoothed IQ points.

Kagan *et al.*'s report (96) analyzed the Rorschach and TAT correlates of IQ change in the Fels children. They found that children with maximum increases (upper quarter of distribution of IQ changes) evidenced higher need achievement, competitive striving, and curiosity about nature than the children with maximum decreases (lower quarter). They conclude that personality needs may influence intellectual growth by facilitating skills measured by the intelligence test. "However the exact relationship between genetic variables and IQ change has yet to be determined. The phenomenon of IQ increase during the school years is admittedly complex and it is not implied that the child's motives are the major factor."

Sontag *et al.* (157) showed that more Fels boys than girls were found to have accelerative patterns in their trend lines, particularly during the school years. Ratings made at age six which differentiated the ascenders and descenders in IQ over the next four years were aggressiveness, self-initiation, and competitiveness. It is inferred that progress in "learning how to learn" is correlated with increments in IQ. Emotional dependence on parents for children between 3 to 6 years of age is considered detrimental to intellectual growth. However, the child who learns to meet some of his needs through aggressive, competitive problem-solving is laying a groundwork for need achievement. "During the elementary school years it is therefore not surprising to see a high need for achievement being related to an accelerated mental growth rate."

What have Sontag and his associates really demonstrated in showing a relationship between need achievement and accelerated gains in achievement on the Stanford-Binet test? It cannot be denied that the Stanford-Binet is an achievement test especially constructed to minimize individual differences in social background and schooling. Is one of the basic assumptions of intelligence testing violated if there are large individual differences in the needs related to the achievements measured by the test? Allison Davis has argued for several years that lower-class children do not have as strong needs as middle-class children to learn answers to the problems pre-

sented in most of the current intelligence tests. Are they thereby less intelligent in the most meaningful sense of this concept? Can intelligence tests be "culture free" of motivational differences and still serve as useful predictors of academic success and life accomplishment? The Fels study has laid the groundwork for a lively controversy, and may stimulate some valuable research into the nomological network which supports the broad concept of intelligence.

Honzik (86) threw some new light on the relative effects of nature and nurture on intellectual growth. She found that whether children are reared by true or foster parents the correlation between their true parents' intellectual indices and their own remains the same. Correlations between .35 and .55 are obtained by the time the children reach 4 to 6 years of age; then they tend to stabilize. The correlations between children's and foster parents' intellectual indices approximate zero, verifying the findings of the earlier Skeels and Skodak study. Honzik concludes that the education of parents per se is not an important factor in determining parent-child resemblances in intellectual status. The correlations between the intellectual brightness of children and their true parents (obtained even when the children are living with foster parents) "reflect individual differences which are largely genetically determined."

Burt (31) also joined the nature-nurture fray with a report of some highly interesting data on the intellectual growth of identical twins separated early in life. He collected 30 sets of these twins in England where he reports the practice of separation is fairly common. The correlation between the intellectual status of identical twins reared together is approximately .92, as contrasted with approximately .85 for identical twins reared apart. The correlation between the over-all school achievement of identical twins reared together approximates .90, while the correlation for identical twins reared apart is only .72 (slightly lower than the .77 obtained for siblings reared together). Intellectual growth, according to his interpretation, is little influenced by environmental variation within the average English home. Academic achievement is substantially influenced but not intelligence. He judges from the findings of his own research, based on "adjusted" intellectual assessments, that only about 12 per cent of the total variance in level of intelligence is due to nongenetic influences. He advances the theory that intelligence is inherited via Mendelian factors of two main overlapping kinds: major genes which produce large deviations and multiple genes "whose effects are *small, similar and cumulative*." The nature-nurture question was considered in a less partisan (and less interesting) fashion by Maddox (116).

During the year there were two follow-up studies of adults classified as mentally deficient or educationally subnormal during childhood. Collmann & Newlyn's study (40) interviewed 190 cases, or their relatives, living in England. They concluded that with few exceptions these ex-pupils of special

schools for the educationally subnormal were fairly successful in attaining the civic and social standards of their communities. Charles (37) found that the adult adjustments of 206 mental deficient were less successful in this country. Their health and vitality were inferior, and the males were often involved in social and legal conflicts. However, 80 per cent were generally employed, married, and produced slightly more than two children per family. Only 5 per cent were in institutions.

Tests of intellectual ability.—The problem of testing intellectual ability falls outside the scope of developmental psychology. However, the meaning and usefulness of intelligence tests are often extended by studying their developmental and sociocultural correlates.

Woods & Toal (178) examined the subtest scores of 120 matched pairs of Negro and white adolescents on the Revised Beta Test. The 14- to 17-year-old subjects were matched on sex and total scores on the Beta. They found the Negroes superior on subtests requiring perceptual speed and accuracy. They performed less well than the whites on subtests 3 and 5 which in the authors' opinions are "culturally loaded." Laird (106) obtained evidence that socioeconomic status has a significant effect on the performance of 11-year-old boys on the full scale and verbal IQs of the WISC. Sperrazzo & Wilkins (158) administered the Colored Progressive Matrices Test to 480 children from 7 to 11 years of age. They found the norms established by Raven adequate for this sample, although there was some indication that the ceiling of the test was too low for this group of average children. The Matrices Test correlated .23, .30, and .40 with the Otis, California, and Kuhlmann-Anderson, respectively. Hiskey (84) administered the Nebraska Test of Learning Aptitude (originally designed for the deaf and hard-of-hearing) to a group of 400 children with normal hearing and intelligence. Scores on this test correlated .83 with the Stanford-Binet.

What is the most useful scale of intelligence for predicting the behavior of mentally retarded children? Evidence over the years has shown that these children respond very differently to the available tests. Sharp (151) compared the performance of 50 slow-learning children on the Stanford-Binet, WISC, and Leiter scales. Although the mean differences were not statistically significant, the intertest variation was at the .001 level. The author questions the adequacy of the Leiter for use with older mentally retarded children because of its extreme variation. May & Perry (121) studied the relationship between the vocabulary test of the Stanford-Binet and the Columbia Mental Maturity Scale within a group of mentally retarded children. A significant correlation of .43 was obtained, a fairly high correlation considering the restricted range and reliabilities of the two instruments. Hunt & Patterson (88) obtained some evidence that mentally retarded boys perform better on the Draw-a-Man test when motivated by an extrinsic reward.

TRENDS IN PERSONALITY ORGANIZATION AND FUNCTIONING

Personality persists as an amorphous yet necessary concept in developmental psychology. The variety of attributes and functions subsumed under its umbrella is unmistakable evidence of its many meanings to different scholars. To many it is a programmatic conceptual model that integrates the many dimensions of overt and covert behavior. To others it is a sort of limbo for behaviors that yet lie outside the boundaries of traditional nomenclature. To still others it is synonymous with the structural and dynamic properties of psychoanalytic theory. All of these viewpoints are well-represented in the research of the past year.

Studies with roots in psychoanalytic theory.—Beloff (13) factor analyzed questionnaire data from 120 undergraduate students and found one general factor of "anality" among the many anal characteristics studied. A factor analysis of peers' impressions of these subjects also yielded the same general factor. A comparison of the bowel training histories of 43 post-graduate students (records obtained by interviews with their mothers) and their anal characteristics showed no significant relationships. However, the anal characteristics of the mothers, as reflected in their responses to the questionnaire, were significantly related to the anal characteristics of their offspring. "From our data then we may conclude that although the anal character is a meaningful dimension of variation for the description of our subjects' attitudes and behavior, it is not related to toilet training experiences, but strongly to the degree of anal character exhibited by the mother." Beller (12) also found some substance in the descriptive concepts of anality and orality. Eleven teachers rated 51 children (28 to 74 months of age) on anality, orality, dependency, and autonomous achievement striving. The core anal characteristics (spilling, smearing, lack of bowel control, etc.) were positively and significantly related to orality (biting, spitting, over-eating, etc.) positively but weakly related to dependency, and inversely related to autonomous achievement striving. The core oral items were positively related to dependency and inversely related to autonomous achievement striving.

The possible relationship between early training and personality characteristics gained support from other investigators. Anthony (6) on the basis of an extensive review of the literature and clinical findings, confirmed the relationship between early toilet training and the bowel dysfunction of encopresis. Allinsmith (3) reported a substantial correlation between intensity of guilt over hostile feelings and age of weaning and bowel training. The results suggest that it is possible to predict from certain early experiences the moral feelings of children 12 years later. The laying of the ghost or the crowning of the hero awaits more definitive evidence.

The personality traits approach.—Cattell & Coan (35) obtained behavior ratings from the parents of 145 first- to second-grade children. The inter-correlations among these ratings and certain other variables were factor

analyzed and reanalyzed (with the more adequate communality estimates from the first analysis). The authors concluded that the personality factor structure in these young children does not appear noticeably less complex than that for adults. Certain habit collections are specifically associated with the sex of the child. For example, femininity seems to produce a component similar to the presia or sensitivity factor (cautious, very retiring, very neat, orderly, etc.). The youngest position in the family tends to help the formation of the surgency pattern (talks a great deal, has a happy cheerful disposition, etc.). Cattell & Coan (34) also factor analyzed teachers' ratings of 198 first- and second-grade pupils on 38 personality variables. They concluded that every factor but one previously found for older subjects can be identified with considerable confidence in these young children. One wonders how young the subjects would have to be before a substantial number of the identified factors would fail to make their appearance.

Cattell, Stice & Kristy (36) tackled a most difficult problem via multiple-variance analysis. They attempted to determine nature-nurture ratios for 11 primary personality factors by analyzing the personality test responses of substantial numbers of identical twins, same-sex fraternal twins, same-sex siblings reared together, same-sex siblings reared apart, and unrelated same-sex pairs reared together. Their findings showed heredity to be the main determiner in intelligence and "comention" (gregariousness, honesty, and acceptance of social and ethical values.). Environment was the main determiner in assertiveness (similar to need-achievement), corticalertia (speed in simple nervous responses), neural reserves-vs.-neuroticism, self-sentiment control, asthenia (social-climber qualities), and over-responsiveness. Heredity and environment were about equal determiners of inhibition, critical practicality, and exuberance. The authors regard these findings as being consistent with the results of previous studies.

The consistency of personality functioning.—Does the human personality set a firm course which permits prediction over long periods of time? Perhaps it does in some behavior functions but apparently it does so very slightly in others. Beilin (11) collected self-ratings and teacher evaluations on 3200 children between the ages of 9 and 18. Predictive indices based on these data were computed and related to criterion measures of adjustment four years later. The obtained correlations were discouragingly low ($-.18$ to $.41$). Tyler (169) found similar distressing low relationships over time in the patterning of measures of adjustment during adolescence. Perhaps the adolescent years constitute an unfavorable time to look for stability and consistency. At least Dunbar (57) implies that a greater amount of physiological stress and ill-considered motor activities prevail during this period of life.

A minor but interesting note on behavior consistency during early childhood was presented by Allen (2). The Rorschach test was administered at

three-month intervals to a girl, beginning when she was two-and-a-half years old and continuing until she was five-and-a-half. Various intelligence tests were also administered at intervals. Her IQ was stable around 130 on the Binet, Leiter, and WISC. One Rorschach protocol showed a wide discrepancy with her intelligence test performance. On the Rorschach she displayed low productivity, low manifestation of drive and imagination, and seemed unusually reality bound. This protocol was obtained at the time she was being introduced into a preprimary school situation. The author speculates that this experience during which the other youngsters were less permissive and tolerant than her parents caused her to "pull in" a bit and to become overly cautious and objective. "The major inference, evidenced early in this longitudinal study, is that extreme caution must be used in the interpretation of a single Rorschach record as representative of the individual's personality structure and dynamics."

Physical growth correlates of personality.—Mussen & Jones (131) compared the TAT protocols of 17-year-old boys who had been consistently accelerated in physical growth throughout adolescence with those who had been consistently retarded. Their responses were analyzed according to a scoring schema of 20 needs, press, and descriptive categories. The physically retarded boys were found to have more negative self-conceptions, feelings of inadequacy, strong feelings of being rejected and dominated, prolonged dependency needs, and rebellious attitudes toward parents. The two groups did not differ significantly in need for achievement or for personal recognition. The conclusion is drawn that rate of physical maturing may affect personality development in crucially important ways.

Davidson, McInnes & Parnell (44) studied the distribution of personality traits in 7-year-olds by a combination of psychological, psychiatric, and somatotyping techniques. Boys with less than average muscular development tended to display more anxiety and "showing off" behaviors. Those with above average fatty tissue were more self-confident. The stocky boys possessed superior abilities to communicate and greater aggressiveness. According to these findings, there may be consistent temperamental correlates of physique during childhood similar to those hypothesized by Sheldon during early adulthood.

Parental influences on personality growth and adjustment.—Hawkes, Burchinal & Gardner (79) correlated scores on the Rogers Test of Personality with family size, and failed to find significant differences in favor of children from larger families. It is not surprising that family size per se is unrelated to children's adjustment status. Adopting a more psychological approach, Altman (5) found that on the Rorschach children of "contented" mothers, as contrasted with "discontented," evidenced significantly more intellectual and emotional freedom, and better anxiety control. Harrington & Hassan (72) noted that splitting of the mothering function during the first two years of life tended to be followed by ego weakness and self-

depreciation during the 8- to 11-year latency period. Strong feminine identifications were depressed in these girls. Fathers are also judged to be of importance in personality formation. Eisenberg's (58) analysis of the fathers of autistic children revealed evidence of serious personality difficulties that impaired their assuming a normal parental role. Their deficiencies were judged to have a negative influence on the pattern of family living.

Donvan & Adelson (55) used open-ended and projective questions in a 1- to 3-hr. interview with 1000 14- to 16-year-old boys to investigate their social mobility aspirations as compared to their fathers' positions. Upward stable, and downward mobile patterns were identified. Donvan & Adelson concluded that a relatively punitive style of parental discipline produced a mixture of overt dependency and covert aggression toward parents. The boys from these homes tended to be downwardly mobile in their aspirations. They seemed relatively humorless, gauche, and disorganized, with impoverished self-governing controls. In contrast the upward aspiring boys were characterized by autonomy, social maturity, and high energy level. These positive personality characteristics were attributed to their successful resolution of infantile object-ties and conflicts. Siegel's (154) clinical report on a 13-year-old fire setter also reflected the same confusions and ambiguities of the downwardly mobile boys. The fire setter's mother told him to "act good" but covertly encouraged him to behave otherwise, just as her father had done toward her.

Personality and behavior disturbances.—Freedman & Bender (62) conducted a follow-up study of six males who were classified as schizophrenics during the prepubertal period. All of them are still so classified at ages beyond 21 years. Although many of their childhood schizophrenia problems had been modified, most of these persisted into adult life. Five of the men are now getting along in their communities but with some difficulty. Michael, Morris & Soroker (126) did a follow-up study of 164 subjects who 26 years previously had been classified as introverts on the basis of their clinical histories. They found no support for the hypothesis that introverted children are more likely than extroverts or ambiverts to develop schizophrenia in later life. Alpert (4) presented a case history of a three-and-a-half-year-old child diagnosed as schizoid. Therapy oriented around a special teacher who provided a constant and consistent need-satisfying relationship produced dramatic gains in personality organization. The efficacy of the therapy was attributed to the building and strengthening of deficient ego functions.

School phobia attracted some attention as a personality disturbance. Talbot (165) found 24 such cases suffering from anxiety neurosis. They were clinging to their mothers both physically and psychologically. Coolidge, Hahn & Peck (41) noted two trends in personality type among 5- to 12-year-old children with school phobias: neurotic, in which the symptom is in the context of an otherwise fairly sound personality and where the onset is

acute and dramatic; and characterological, in which the onset is less acute and the symptom is only one aspect of a more general and insidious disturbance. The mothers of the children in both groups encouraged dependency. On the basis of a study at the Judge Baker Clinic of 53 cases of school phobia, Waldfogel, Coolidge & Hahn (170) concluded that such children's anxieties stem from fear of being separated from the mother. The typical mother in this group handled her child's anxiety by overprotectiveness. She was closely identified with her child and used him excessively as a means of vicarious gratification. In none of these reports is there any indication that any special conditions in the classroom were related to the children's difficulties.

An offbeat study by Spivack (159) examined the child-rearing attitudes of emotionally disturbed adolescents who were being treated in a residential center. Their attitudes were compared with those of a control group of normal adolescents in a residential school. The emotionally disturbed subjects of both sexes expressed a significantly more "restrictive" controlling attitude. The conclusions are drawn that child-rearing attitudes of over-control and restriction are perpetuated, and that emotionally disturbed adolescents are not rebelling but rather searching for self-definition and standards of conduct to follow. Jenkins (92) also conceived delinquency as a response to chronic frustration. In these individuals the goal-motivated response of delinquent behavior occurs most frequently where there are opportunities to observe and participate in the use of delinquent techniques. The search for self-definition is presumably successful, albeit socially unacceptable.

Testing personality and adjustment.—One interesting new instrument designed for the developmental study of personality was introduced during the year, and several of the old ones were re-evaluated. Haworth (80) filmed the puppet play "Rock-a-bye, Baby" and showed it in small groups to 250 children between 4 and 10 years of age. The film was shown up to a dramatic point where it was stopped and the children were asked to finish the story. Then the remainder of the film was shown and the children were asked individually a set of questions designed to tap various attitudes and feelings. After norms were established the effects of a younger child in the family were investigated among the 6-year-olds. Significant differences were obtained with respect to the baby and to the parents in the groups with and without a younger sibling. Several indices and scales were developed: a sibling rivalry index, an autoerotic index, and a superego scale. The technique was developed to serve as a quick screening device to identify deviate cases for further clinical study and treatment.

Furuya (63) investigated the relative productivity of responses to an equal number of animal and of human pictures among first-, fourth-, and sixth-grade children. The results showed a significantly superior productivity in response to human pictures at all age levels. It appears that chil-

dren as young as 6 years identify better with, or are stimulated more to verbal expression by, human characters. Setze and associates (149) conclude that another embellishment in testing procedure for young children is also unnecessary. They found that the Ford trial blot method, recommended as a nonverbal orientation to the Rorschach test, did not influence the responses of 6- to 8-year-old children. Levitt (112) also threw water on the flames of our previous convictions. He compared Rorschach indicators of anxiety with scores on the Manifest Anxiety Scale and concluded that "... only frequency of shading responses can be considered to be an anxiety indicator in children."

Burchinal (29) would have us discontinue our efforts to find relationships between parent attitudes toward children (as measured by the Shoben and the Porter scales) and their children's personality and adjustment status (as measured by the California and Rogers tests). Among mothers the two parental attitude scales were unrelated, and among fathers the relationship was only .32. All of the 16 correlation coefficients between parental attitudes (as measured by the Porter scale) and children's personality scores were nonsignificant. Fourteen of the 16 correlation coefficients between parental attitudes (as measured by the Shoben scale) and children's personality scores were nonsignificant, and two were significant in a direction opposite to expectation. Burchinal has ample reason to be discouraged with this orientation toward parent-child inquiry.

DEVELOPMENTAL TRENDS IN SOCIAL BEHAVIOR

The genesis and ontogeny of social needs are largely obscure in contemporary psychology. Some theorists have proposed that the major social tendencies in man are genetically determined and become manifest according to a neurophysiologically controlled time schedule. Others regard these needs as having their being on the basis of learned responses. Whatever their backgrounds, it is clear that instrumental behaviors related to social-needs satisfaction are heavily influenced by conditioning and learning. Therefore, it is meaningless to discuss the development of social behavior without simultaneously specifying the relevant environmental conditions. Many of the investigations in this area are attempts to identify significant environmental correlates of social response.

Social acceptance and related variables.—McCandless & Marshall conducted a series of excellent studies on correlates of social acceptance among children in the University of Iowa Preschool Laboratories. On the basis of a thoughtful survey of the research literature, Marshall (118) found no good evidence that sociometric choices predicted social acceptance or interactions among preschool children. Then McCandless & Marshall developed a new sociometric instrument (described elsewhere in this review) and applied it in several investigations. Marshall & McCandless (119) demonstrated that social acceptance in spontaneous play is related to both sociometric and

teacher-judgment measures, and that sociometric scores are significantly related to "Best Friend Scores" derived from observations (r s between .34 and .58). Since the reliability of the Best Friend Scores appears to approximate .70 and the stability of the sociometric scores is near .60, these relationships between sociometric and observational indices would appear to be substantial when corrected for the attenuating effect of errors in measurement. Marshall & McCandless (120) also showed that dependence on adults in the preschool situation is associated with relatively low social status and participation. McCandless & Marshall (123) found that the sociometric scores of preschool girls averaged about one-third higher than the scores for boys. The majority of the negative relationships between dependency on adults and social acceptability were also significantly larger for girls. The authors discuss some of the possible reasons for these findings.

Koch (103) contributed another report in her sustained and thorough investigations of the family-structure correlates of social behavior. Among other things she found that first-born children played more often with children younger than themselves than did second-born children. Although children in general preferred same-sex playmates, children with an opposite-sex sibling more often chose a preferred playmate of the opposite sex. It seems clear that the age and sex of siblings do influence a child's associates and playmate preferences.

Gallagher (64) added to the now mounting evidence that social popularity is positively related to intelligence, and that propinquity favors friendship. Devault (53) also supported the general conclusion of previous studies that physical proximity plays a less decisive role in the friendships of older children. There was a curious reversal in this trend among tenth- to twelfth-grade pupils, which may have been due to special social conditions in this sample.

Rosenthal's (142) findings suggested that language facility (with IQ controlled) may favor social acceptability among second-grade children. Davis (45) found the most popular eighth-grade boys to be better readers, more intelligent, better adjusted, and to have more favorable attitudes toward school. It is difficult to estimate whether or not intelligence may be the common denominator for all these relationships. Trent (168) found that institutionalized delinquent boys who were most frequently chosen in a sociometric situation were also most frequently rejected. The more "anxious" boys were less popular but no more frequently rejected. The possibility is suggested that anxiety may not lead to rejection but may hinder the attainment of popularity. Allen & Masling (1) found that children with nursery school experience enjoyed a greater amount of social acceptability in the primary years than children without this experience. Since social maturity, parental education, age, and sex were controlled, it would appear that some nursery school experiences may promote social skills important to popularity. However, on the basis of a careful survey of the research

literature, Bonney & Nicholson (19) show that nursery school experience per se will not insure a positive carry-over to the later school years. Lapp (108) presented evidence that slow-learning children assigned part-time to regular classes are merely "tolerated." They obtained both low acceptance and rejection scores which may reflect these children's "passivity."

Ascendance and leadership.—Stott & Ball (162) rated 60 children on 30 items of an ascendance-submission schedule at varying periods of time over a 10-year interval. Interactive behavior of any type was least frequent at the 3-year-old level. It doubled by age 5, dropped again at entrance to kindergarten, and then remained fairly stable to 12 years with a slight increase during the ninth and tenth years. The increase in interaction with peers was largely a change in degree of ascendance. The relative amounts of domineering bossiness, leadership, and undifferentiated ascendance did not change in any significant way with increasing age. Proceeding along somewhat similar lines, Gold (68) studied the social "power" of children in elementary classrooms. The power positions of boys and girls seem to be related to their values and social-emotional properties.

Marks (117) conducted an interesting study of the interests of leaders in a large high school population. A leader and a follower were selected from each of 21 boys' cliques and 28 girls' cliques. It was found that girl leaders were significantly higher in science interests, attractiveness, popularity, prestige prominence, style setting, and athletic leadership—described as "big wheel" characteristics. Boy leaders were significantly higher in social interests, popularity, acceptability, prominence, and athletic leadership—called "good guy" qualities.

Aggression and hostility.—The study of aggression and hostility via play techniques and projective devices has attracted the interests of many psychologists. The determinants of the aggressive response continue to be revealed. Siegel (152) observed the social play of like-sexed pairs of nursery school children in two sessions one week apart. In both sessions the children were free of adult supervision or intrusion. The incidence of aggression, anxiety, and guilt decreased from the first to second session. This finding is the opposite of that obtained in previous studies of play where an adult was always present. The author speculates, "In the presence of an adult experimenter, young children may abdicate superego functions to him, whereas in the absence of any adult their own internalized standards increasingly resist expression of unacceptable drives, with a consequent reduction in anxiety and guilt." Whatever the source of social influence on the functioning of internalized standards, Livson & Mussen (114) showed that nursery school children with greater ego control display less overt aggression in their social interactions. Ego control was inferred from behavior in delayed-gratification and co-satiation situations.

Lebo & Lebo (109) cast some doubt on the generalization that children who are able to express overt aggression in everyday life will have less need to display aggressiveness during nondirective play therapy. They found that

children 4 to 12 years of age judged most aggressive by their teachers were the most aggressive during three 1-hr. sessions of nondirective therapy with the same therapist in the same playroom. The children rated more aggressive by their teachers made more aggressive statements and threats to playroom rules, made more favorable statements about themselves, and tried harder to establish a social relationship with the therapist. Aggressive verbalizations were most frequent in the 6-year-old group and then steadily declined with increasing age. The authors believe that their findings require "explicit consideration in the formal theoretical structure of nondirective play therapy."

Jensen (93) established some interesting relationships between the fantasy aggression of TAT themes and different type of overt aggression. On the basis of teachers' ratings high school boys were divided into subjects who were aggressive in socially acceptable ways, aggressive in socially unacceptable ways, and not aggressive. No direct relationship was found between fantasy and overt aggressive classifications. In fact the "aggressive-good" group had even less fantasy aggression than the passive group. The "aggressive-bad" group displayed a greater proportion of "raw" aggression, and was far in the lead on themes of sex, tabooed sex, tabooed language, and tabooed violence, but low in themes of punishment and defenses against aggressive anxiety. The aggressive-good group had more themes of natural death, interpreted as a defense against aggressive anxiety. It is concluded that subjects who habitually "act-out" their aggressions in tabooed ways within the school setting also respond to the TAT with socially tabooed content and language. Shapiro (150) believes that aggressive children have learned that aggression may lead to reward, either in the attainment of goals or in the reduction of anxiety. Withdrawn children have learned that social withdrawal and renunciation of goals are most effective in allaying anxiety. These inferences were based on the perceptual responses of aggressive, withdrawn, and well-adjusted boys (6 through 10 years of age) to family situations and family drawing tasks. Kaplan & Goodrich (97) also concluded that the "anger" response does not occur as an invariant response to frustration, but depends upon the individual's interpretations of the behavior of others.

The growth of sex-social roles.—It appears that girls have a more difficult time achieving an appropriate sex role than boys. Gray (70) administered a masculinity-femininity scale (devised for this study), the Manifest Anxiety Scale, and a social-acceptance instrument to 34 boys and 27 girls in the sixth and seventh grades. Children high in anxiety were found to be significantly more sex-appropriate in behavior than those low in anxiety. Although high social acceptance was associated with a high degree of sex-appropriate behavior in boys, such was not the case with girls at this age. "Where the tomboy is as well liked as the 'young lady'—and sometimes better—it is difficult indeed to know what role to follow."

Brown (26) investigated the masculinity-femininity of younger children

(five-and-a-half to eleven-and-a-half years of age) with a projective-type test. Again it was found that girls do not show nearly the same degree of preference for the feminine role that boys show for the masculine. In fact, during the earlier years the girls show a stronger preference for the masculine role. Is this a reflection of the "masculine protest" among relatively naive subjects? Or is it, as the author suggests, due to the relatively greater freedom of girls to adopt male roles in dress, play, etc.? Brim's (23) analysis of some of Helen Koch's data suggests the latter interpretation. He found that when personality traits are classified as masculine (instrumental) or feminine (expressive) more traits of the opposite sex are found among children with cross-sex siblings than with same-sex siblings. He also found that this effect was most pronounced among the younger siblings. Social reinforcements of sex-appropriate behavior would appear to be an influential variable in the acquisition of masculine and feminine roles. Goodenough (69) presented some additional data that support Brim's classification of personality traits according to sex-roles. Goodenough found that nursery school girls draw more pictures of and mention persons more often than boys of the same age. Interviews with their parents reflected the fathers' intense interest in the differentiation of boys' and girls' activities, and the mothers' expectations that there will be differences. The author believes that girls are rewarded more frequently for personal orientations and the boys more often for objective, and less personal, orientations.

SOCIAL AND CULTURAL CONDITIONS RELATED TO PSYCHOLOGICAL GROWTH

Broad cultural and social influences.—From a survey of 110 cultures, Barry, Bacon & Child (9) conclude that socialization pressures directed toward differentiating the sexes are minimal during infancy. In our own culture there are somewhat greater pressures toward nurturance, obedience, and responsibility in girls and toward achievement and self-reliance in boys. The authors wonder whether there may now be fewer differentiating sex-role experiences during childhood than are required for adequate preparation for adult life. "... we have compelling reasons to expect that the decrease in differentiation of adult sex role will not continue to the vanishing point."

Dennis (48) used the critical incident technique to study the behavior for which American and Near-Eastern children between the ages of 5 and 11 years expected to be praised. The American children expected more praise than the Near-Eastern Arabs, Armenians, and Jews for giving and sharing, performance in sports and games, creative activities, and for assisting persons other than unfortunates. The Near-Eastern children expected more praise for academic achievements, for assisting unfortunates, and for being quiet, polite, and obedient. Dennis (50) also obtained evidence for another type of differential reinforcement between Near-Eastern and American children. He found a steady decline from 5 to 10 years in IQs

on the Goodenough Draw-a-Man Test among certain Near-Eastern children. In Port Said the decline was from 94 to 80, in Sidon from 104 to 62, and in two Armenian groups from 115 to 95 and from 84 to 74. Since the children attending a Near-Eastern university school did not show a decline, he believes that the declines in the other groups may be due to a cultural lag in shifting from former taboos against representations of the human figure. In these cultures there are still very few dolls, masks, wood carvings, sculpture, paintings, or other pictorial forms.

Barry's (8) investigation of severity of child training and complexity of artistic style in 30 nonliterate cultures reveals one aspect of interrelatedness in acculturation. The greater the complexity of artistic design, the greater is the severity of socialization practice. It almost seems as if the greater the complexity and difficulty of the "developmental tasks" in a culture, the greater the anxiety of parents that their children may fail to meet the cultural requirements. Werner (172) showed that it is hazardous to evaluate social competence by the same criteria, even within the subcultures of our own country. On the Vineland Social Maturity Scale urban children were most socially competent at the kindergarten level, and rural children were the most self-reliant at the fifth-grade level. An examination of the items in this test leaves little doubt about the reasons for these subcultural differences.

Phelps & Horrocks (135) found that to some considerable degree adolescent youth are able to establish a subculture of their own. The authors factor analyzed the responses of 200 adolescent youth from a central Ohio community and established 10 factors in their recreational activities. They did not find socioeconomic status to be a dominant factor in their informal group attitudes, but did find the public school a strong influence in promoting upper socioeconomic values. The degree of emancipation from adult control was also an important variable in the formation of patterns of group activities and attitudes.

General parental influences.—Mussen & Kagan (133) compared male college students' perceptions of parents in response to eight TAT cards with their conforming behavior in the Asch experimental situation. They found that the extreme conformists tended to perceive their parents as harsh, punitive, restrictive, and rejecting. These findings were interpreted as suggesting that conformity tendencies are manifestations of basic personality structure, influenced by early parent-child relationships. Psathas (137) obtained evidence that lower-class parents were more permissive toward their adolescent offspring and that middle-class parents supervise the activities of their adolescent sons and daughters more carefully with the result that the children hold them in higher parental regard. In view of the findings of these two studies one might expect to find fewer conformity tendencies in lower-class adolescents—a conclusion that seems to parallel superficial observations. Drews & Teahan (56) presented another finding

consistent with these observations. They found that the mothers of high achievers were more authoritarian and restrictive in terms of their children's intellectual abilities than the mothers of low achievers.

Cole *et al.* (38) interviewed a sample of 200 families within the urban confines of Salt Lake City. They found that these predominantly Mormon parents were gradually assimilating recent research and clinical findings about child-rearing practices. The younger parents seemed to be most alert to new procedures. It would be interesting to know just how much the parents in a broad sample of our culture have been influenced in their child-rearing attitudes and functions by contemporary psychology—and in what directions!

The mother's influence.—Child rearing has long been regarded as a primary function of mothers. To some it is a private domain with little trespassing encouraged or permitted. There can be no doubt that the mother has a profound effect on the behavior and development of infant and child, and vice versa. This state of affairs is fortunate for the developmental psychologist, because the mother is far more accessible for study than the father. This does not deny that the latter's influence may have been seriously underestimated because of his relative inaccessibility to the research worker.

Schaefer & Bell (146) extend the hope that it may be possible to predict the course of mother-child interactions from attitude data gathered prior to the birth of the child. They did a factor analysis of the responses of 100 student nurses to 24 Likert-type parental attitude scales and identified the following independent factors: suppression and interpersonal distance, hostile rejection of homemaking role, excessive demand for striving, over-possessiveness, and hostile punitive control. It now remains to be seen whether the same factors would emerge in another sample of postadolescent females and whether these attitudes are significantly modified by the child-bearing event.

Hart (76) related degree of authoritarianism among mothers (as reflected in the F scale and TFI items) to their use of love and nonlove orientations toward such child-rearing practices as feeding, cleanliness, toilet training, sex, dependence, and independence (as estimated from interviews). The more authoritarian mothers selected more nonlove-oriented and fewer love-oriented controls than the nonauthoritarian mothers. Aggression and independence provoked more nonlove-oriented responses from all the mothers (physical punishment, threats, and ridicule).

A series of studies illustrate some of the dynamics of the mother-child interaction. Lesser (110) found that sons of mothers who encourage aggression display similar amounts of overt and fantasy aggression (r of .43). Sons whose mothers discourage aggression evidence a reverse relationship between overt and fantasy aggression (r of $-.41$). Levin & Turgeon (111) report the usual finding that boys are more aggressive than girls during doll play. They also found that children increased their aggressive responses in

the second session of doll play when the mother was present, and decreased these responses when a stranger was present. The authors speculate that self-control "is a worrisome burden for a child" which he is happy to transfer to mother when she is present. They also venture the opinion that the children's aggression would have decreased in the mothers' presence if the mothers had been able to discipline their children. The latter hypothesis could be tested under varying degrees of maternal control. Morgan & Gaier (128) used a group of projective pictures to investigate the reactions of mothers and 9- to 12-year-old children to various punishment situations. Their responses to predetermined questions were scored for ego defense, obstacle dominance, and need persistence. They found a tendency for both mothers and children to overestimate the ego-defense reaction in the other person but not in the self. Mothers regard themselves as seeking a solution to the situation more frequently than children perceive them as so doing. By and large, children fail to recognize the extent of situational obstacles for their mothers. Mothers who may seek to arouse the sympathies of their children in behalf of their child-rearing roles can take little comfort from these findings.

The effects of "minimum mothering."—How fares the Spitz hypothesis on the dire effects of "mother deprivation" on child growth? Not too well, in the opinion of Dennis & Najarian (52), who found no emotional upheaval in institutionalized children in Lebanon although they enjoyed little "mothering care." These children came to the institution shortly after birth and remained until 6 years of age. Their contacts with mother substitutes were slight because the adult-child ratio is about 1 to 10. The infants were swaddled, lay on their backs, and were even fed in a supine position. Although these infants showed extreme retardation on the Cattell infant scale between 2 and 12 months of age, they had normal IQs on the Goodenough, Knox cube, and Porteus tests at older ages. There was no evidence of any kind of "emotional shock." "It is believed that the objective data of other studies, as well as this one, can be interpreted in terms of the effects of specific kinds of restrictions upon infant learning."

Hopper & Pinneau (87) also landed a blow against Ribble's hypothesis. They found that the mother's stimulation of the infant prior to feeding (handling, rocking, swinging, rolling, etc.) did not produce a significant decrease in burps and regurgitations after feeding. Since there is a decrease in regurgitation with age, they suggest that Ribble's findings and conclusions may have been due to the lack of a control group.

Rabin (138) and Faigin (60) found no ill effects on the development of children in the *Kibbutz* (Israeli collective settlement) where there is communal living and an absence of "continuous mothering." Rabin concluded that their Rorschach protocols do not reflect any deleterious effects due to the alleged early maternal "deprivation." There was even some evidence that the *Kibbutz* children had greater personality maturity than the control chil-

dren. Faigin concluded that group identification, sharing, and group control of individual behavior are learned by the *Kibbutz* children during the early preschool years. The Spitz and Ribble hypotheses had a bad time of it in this year's literature.

The father's influence.—To judge by the bulk of the research, the father works hard all day and comes home to an empty house—important only for procreation and the economic support of his wife and offspring. On the basis of 79 reports on parent-child relations, Brim (24) worked out a rubric of different family interactions and roles. Entries in the father cells were sparse. Pierce-Jones & Littman (136) in a note on their new research program in Eugene, Oregon, observe that "Fathers seldom have been studied as extensively as mothers or by the same methods." They have plans to correct this condition in their study. Burchinal (28) found that fathers were less accepting of children than mothers (Perhaps they feel left out of the picture by sociologists, psychologists, and psychiatrists!). Katz (98) spoke out in the defense of the father's importance in child rearing, but his was the voice of the past—a rabbi quoting proverbs.

TEXTBOOKS AND OTHER DIDACTIC VOLUMES

One textbook on the psychology of early childhood (107) and three on adolescent development (16, 65, 94) appeared during the year. Two books of interest to parents of infants and preschool children (174, 175), and two of interest to parents of adolescents (140, 163) were also published. Two well-established textbooks were published in revised form (125, 173), and one book was published which encompassed the contents of two existing books (139). By the time this review appears all of these volumes will have been reviewed in one or more of our professional journals. Therefore I have refrained from making the terse evaluations which would be dictated by present space limitations. Although these volumes are not equally sound or helpful by any means, each has some favorable qualities.

SOME IMPRESSIONS

Since it is my opinion that the typical reader turns to the *Annual Review* for factual information in areas tangential to his principal research and scholarly interests, I have tried to present the best of the developmental literature with a minimum of on-the-spot bias. However, one of the rewards for sifting several hundred reports for worthy contributions is the privilege of making a few *ex cathedra* pronouncements. Donning my oracular mantle, I hereby take this privilege. If these be mere projections of my own needs, they will be immediately rejected by readers with different needs and no harm will have been done.

Developmental psychology has come a long way from the time of Roger Barker's pessimistic appraisal of the status of child psychology in the 1951

Annual Review. It seems to have a healthy respect for empirical findings and no longer dreams of an easy conquest by intuitive theory.

Its scientists have largely abandoned what George Stoddard once described as the "scoop shovel approach" to data gathering and have taken to the pick, seemingly more content to gather small specimens from the mother lode. There seems to be a mounting conviction that knowledge about the master plan will eventually be revealed through the efforts of many investigators rather than by a vision to the anointed. Since the history of science generally attests to the wisdom of this attitude, I regard it as an important indicator of greater scientific maturity within the field.

Although well-conceived and skillfully executed investigations continue to come from many different laboratories, the largest number of consistently high quality are completed in the research centers at the Universities of California, Iowa, and Minnesota. Scholars in a large university with research traditions and a chartered purpose continue to conduct the most significant research. There seems to be an object lesson here for philanthropic research foundations.

It is high time for the developmental psychologist to make greater use of infrahuman subjects. The few studies that have been conducted at the Jackson Memorial Laboratory point the way toward a truly comparative approach to developmental principles and theory. In research with human subjects the co-twin methods should be revived for its obvious advantages in genetic control.

The developmentalist's filial relationship to psychoanalytic theory is nearing an end. We are becoming more critical of psychoanalytic postulates and are beginning to doubt the adequacy of much of the supporting evidence. There is an ancient natural law that the son must eventually replace the father.

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LEARNING^{1,2}

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Melton, ten years ago (120), in the first volume of the *Annual Review of Psychology*, discussed the facts of learning in terms of their inter-theoretical implications. Such an orientation would not be appropriate today. It appeared to many, a decade ago, that certain theories, notably those of Hull and Tolman, were engaged in mortal combat in the arena of hard data. Now that the dust has settled, it seems that the combatants were more often shadow boxing. Damage to theoretical positions did occur but in many instances the wounds were self-inflicted. However, these theoretical disputes nevertheless did serve a purpose. They provided much interesting data; but, more important, they revealed the stark inadequacies and limitations of existing learning theories. As a result the tenants of certain theoretical establishments were forced to pay more attention to the development of their own conceptual formulations than to the apparent defects of other theories. Today, most of the work in the psychology of learning is directed solely at developing theoretical formulations (or an atheoretical system), with apparent indifference to the problems of competing theories. This review will attempt to reflect this trend by analyzing the contributions of the past year in their relation to specific orientations. The aim will be to uncover the major developmental trends and problem areas of those orientations that have captured the imagination and allegiance of a sizable group of contemporary psychologists. It is hoped that such an analysis will not only survey the important literature of the past year but also relate it to the future.

It appears to this writer that if there is one influence that dominates most, but certainly not all, of the past year's work in the psychology of learning, it is the formulation of Clark Hull. But the strong influence Hull

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² The following abbreviations and symbols are used in this chapter: r_g (fractional anticipatory goal response); s_g (fractional goal stimulus); S (subject); S-R (stimulus-response); K (incentive motivation); CS (conditioned stimulus); CR (conditioned response); PDR (pupillary dilation response); UCS (unconditioned stimulus); GSR (galvanic skin response); IREE (intermittent reinforcement extinction effect).

exerts on contemporary learning psychology results more from his failures than from his successes. The two most active systematic orientations that compete with the Hullian approach are those of Skinner and mathematical models. One can view the growth of Skinner's influence as a consequence of the dissatisfaction with the empirical base of Hull's theory. Similarly the popularity of mathematical models stems from disenchantment with the formal component of Hullian theory.

There are of course other conceptual orientations in the field of learning. They, too, shall be treated. The extent of their treatment shall be proportional to the amount of work they have instigated.

The assignment of articles to various orientations is not of course a simple sorting task. Authors, fortunately or unfortunately, do not always indicate the conceptual orientation that guided their work. As a result some of the following assignments may be questioned by the reader as well as the author of the original contribution. But in self-defense it should be noted that a year's work in the field of learning represents a disorderly array of facts and hypotheses. Some distortion is a small price to pay for a systematic organization.

NEOBHAVIORISM AND STIMULUS-RESPONSE FUNCTIONALISM

Hullian theory has undergone many changes since Hull's death in 1952. Even during his lifetime he failed to achieve unanimous agreement among his collaborators and adherents about all aspects of his formulation. Since his death many minor changes in and one major (175) reformulation of his theory have appeared. In recognition of these facts the appellation neobehaviorism will be substituted for Hullian theory.

It was the original intent of the author to assign separate sections to the contributions of neobehaviorism and stimulus-response (S-R) functionalism.³ It soon became apparent that it was impossible to detect where neobehaviorism ended and S-R functionalism began. This state of affairs is not due to any tendency for neobehaviorism to consume S-R functionalism or, for that matter, for S-R functionalism to swallow up neobehaviorism. But there is little doubt each is nourished by the facts and ideas of the other. If we are to judge by the sheer number of publications they generate, both neobehaviorism and S-R functionalism must be considered the most vigorous and productive orientations within the psychology of learning.

One can discern changes occurring within both approaches. The neobehavioristic orientation is less united than it was a decade ago. The creative efforts within neobehaviorism range over a wide dimension. At one end is the formal theorizing of Spence (175) (at a much more modest scale

³The rubric S-R functionalism is used in much the same manner as Hilgard (84) used the term functionalism. S-R functionalism refers to an operational S-R orientation that proposes and tests limited theoretical hypotheses.

as compared to Hull) which is concerned with many of the problems that interested Hull. At the other extreme is the more informal theorizing of Miller (124, 125), who fearlessly projects a neobehavioristic model of behavior into many different empirical areas. Between them are the numerous researchers and hypothesizers who apply a modification of a neobehavioristic theory to an empirical area of restricted scope. S-R functionalism has not changed its character, but rather its stature. With its modest inductive-deductive theoretical approach it has expanded its efforts to include a much greater variety of phenomena.

The treatment of the contributions of these two different, but not clearly distinguishable, orientations will be in terms of general problem areas.

MOTIVATION AND LEARNING

There are many facets to the theoretical distinction between motivation (drive) and learning (habit). Some of the recent research and theorizing within neobehaviorism and S-R functionalism can be treated in terms of the implication they bear for the drive and association (habit) concepts, as well as the relationship between the two.

Drive.—Miller (124, 125) has summarized and reviewed a series of fascinating motivational studies that involve psychological, physiological and pharmacological techniques. He has gathered information about how various experimental manipulations of drives influence learning and performance, as well as the drive concept itself. In analyzing the implications of these studies Miller raises a basic theoretical issue: can the concept of drive be shown to have the properties of a single unitary theoretical concept, or must it be broken down into several independent concepts, each involving different clusters of independent and dependent variables, and each possibly possessing different conceptual properties. The research Miller cites suggests, at least, greater attention be paid to this second possibility. The ultimate answer to this question will, however, not be determined solely by experimental results. Apparently discordant findings can often be integrated within a single concept by ingenious theorizing.

The greatest change in Hull's original conception of drive (87) has been the assignment of drive properties to fractional anticipatory goal responses (r_g). Miller (123) assigned drive properties to the anticipation of punishment, Spence (175) to the anticipation of reward objects such as food, and now Amsel (1) has assigned drive properties to the anticipation of frustration. Amsel presents a systematic treatment of this hypothesis [previously mentioned in an informal manner by both Hull (89) and Skinner (167)], which is designed to integrate a number of empirical findings. The theoretical problems associated with explaining the influence of goal box events on instrumental responses that precede them in a chain are difficult to handle. Amsel's proposal is both clever and amenable to experimental attack. But problems still remain. The sharp theoretical distinction between drive and habit is no longer mirrored in experimental manipulations. Ex-

perimental operations that will influence r_g will influence the strength of both habit and drive. Because of this it becomes difficult, if not impossible, to isolate the influences on behavior of these presumably independent theoretical constructs. For example, the results of Holder *et al.* (86), who tested some implications of Brown & Farber's (30) treatment of frustration, are impossible to analyze because of the confounding of the drive and habit variables. It should be noted that some theorists still use the r_g concept without assigning it drive properties (102, 126).

Seward *et al.* (160) question Amsel's frustration drive theory in its application to the results of Amsel & Roussel (2), who had trained rats to run a two-section runway with a pellet at the end of each section. When the pellet at the end of the first section was withheld the rats ran faster to the second pellet. Seward *et al.* report the same results but suggest that the faster running of the frustrated Ss is possibly due to the retarding effect of prefeeding of the nonfrustrated Ss rather than to the motivating property of frustration. Bernstein (12), however, obtained positive results for the frustration drive hypothesis. He found in an instrumental avoidance conditioning situation that short durations of blocking the S from responding to the CS increased the vigor in which the instrumental response was evoked as well as created greater resistance to extinction.

Stein (178) has sought to demonstrate how the classical conditioning of the consummatory eating response and its anticipation (r_g) by the initial stimulus elements of an instrumental locomotor response chain influence the level of incentive motivation (K). He sought to do this initially by influencing K independently of the learning (H) of the instrumental response. He compared subjects who were given direct feedings in a box resembling the alley in which they had been trained with Ss who received direct feedings in a box dissimilar to their training alley. The results of several variations of this technique failed to show the performance jump predicted for those Ss with the higher value of K (i.e., those Ss having the direct feeding in a similar situation). One possible reason for this failure is that the Ss were not given sufficient training in either or both the instrumental and goal responses. Other possibilities are that an intermittent reinforcement schedule (e.g., 204) or some other kind of discrimination technique for either or both the instrumental training and direct feeding experiences would be more effective, or perhaps a Skinnerian technique that emphasizes the behavior of individual Ss would be more appropriate. A final possibility is that the theory does not apply. But this last alternative must be considered by the theorist himself only as the last possibility. Constructive theorizing requires much sweat and tears, as well as stubbornness and persistence.

The applicability of the generalized drive theory to the results of Webb (191) and Brandauer (22) has been questioned by Grice & Davis (72). Webb and Brandauer had trained rats to make a food-rewarded instrumental response. During extinction, when the Ss were food satiated, the

number of extinction responses was found to be an increasing function of water deprivation. Grice & Davis performed an experiment that considered the nonindependence of the hunger and thirst drives (72). Their results indicated that a thirst drive during extinction, contrary to the findings of Webb and Brandauer, exerted a depressing effect on an instrumental food response. Grice & Davis concluded that the generalized drive theory "... may be valid as a general principle but subject to restrictions arising from interaction effect between some specific drive states."

Braun, Wedekind & Smudski (25) report results in a multiunit maze study that are interpreted to be consistent with the generalized drive theory. The influence of drive strength on competing habits depends on the relative strength of the correct response (175). It is questionable whether a multiunit maze is the most appropriate apparatus to investigate implications of the generalized drive theory. Ellis (49), in a runway situation using shock as the irrelevant motivation, could not interpret his results for or against Hull's generalized drive theory.

Ghent (66) points to some of the difficulties of measuring the intensity of drive. Her findings suggest that the intensities of both the hunger and thirst drives are related to the past eating and drinking experiences the animal has had with both needs. Perhaps results of this sort can be interpreted within the incentive motivation theory of Spence (175).

Reinforcement.—More theorists have burned their fingers on the concept of reinforcement than any other. Skinner's atheoretical attitude about reinforcement has gained popularity, if for no other reason than that it keeps people out of theoretical mischief. But there are some brave souls who refuse to give up and therefore continue to speculate about the reinforcement process. Miller (124) suggests two versions of the drive-reduction hypothesis: the weak form states that the reduction in the drive stimulus serves as a reinforcement, while in the strong form reinforcement is always and only produced by drive-stimulus reduction. To some extent Miller keeps out of difficulty because his research explores the feasibility of various theoretical positions without being too strongly committed to any.

Doehring (43) reports an interesting experiment on conditioning passive movement (i.e., "the displacement of part of an organism produced by the action of an agent other than the organism itself"). A white noise was paired with a passive hand movement. During training a number of test trials in which the CS was presented alone were interspersed among the conditioning trials. It was discovered that conditioned muscle action potential responses (involved in the hand movement) occurred. The author suggests that "conditioning appeared to take place with no obvious reinforcement of the response." Two questions can be asked. Is it possible that the CR was mediated by verbal mechanisms? This question could be answered by repeating the experiment with monkeys or perhaps even rats. But what is more important is the meaning of the reinforcement versus contiguity controversy. It was Niels Bohr who observed that physicists

cling tenaciously to the outmoded linguistic usages as long as possible. Psychology does resemble physics in at least one respect! The attitudes of Miller and Doehring toward the reinforcement-contiguity controversy present a study in contrasts. Doehring is to some extent a prisoner of the controversy. Miller's research, on the other hand, is guided but not controlled by the contiguity versus reinforcement issue as it was interpreted over a decade ago. The results of Miller's work (124) are sure to at least modify, if not completely change, the nature of the "contiguity versus reinforcement" controversy.

The implication of pupillary conditioning on the reinforcement process has been brought to the fore again by two carefully executed studies which report contradictory conclusions. Gerall, Sampson & Boslov (65) found it was possible to condition classically the human PDR (pupillary dilation response) when the UCS was shock but not when it was light termination. Young (203) failed to obtain pupillary conditioning in four different experiments. He used a variety of UCS's: change in illumination, convergence and accommodation, and shock.

The present writer in a personal communication asked both Gerall and Young to comment on the reasons for their apparently contradictory results. Gerall was not convinced that Young obtained negative results. He felt that a pupillary CR might have been hidden by high amplitude PDR that the CS (loud bell) evoked prior to conditioning. Also, Gerall notes that the latency of the PDR to the CS was reduced as training progressed, suggesting that some conditioning was occurring.

Young in his communication writes that he is led "... to the conclusion that the results obtained in the two studies are in agreement. . . . I am not willing to conclude (however) that the pupil dilation is conditioned to a neutral stimulus unless the general autonomic response has been extinguished, leaving the pupil to dilate to the CS. . . . I found that when the GSR is extinguished the pupillary response is also extinguished and concluded that the pupillary dilation as such without general autonomic involvement had not been conditioned."

Thus it is seen that the source of the difference between the positive findings of Gerall *et al.* and the negative findings of Young lies not in their empirical results but instead in their definition of conditioning. Young insists that a positive demonstration of pupillary conditioning occurs in the complete absence of any concurrent autonomic responses. In this connection it is interesting to note that Gerall *et al.* agree that their study shows "... that a generalized fear reaction was modified . . . rather than a specific light reflex."

Young is in a certain sense entitled to accept his own definition of conditioning. But his definition leads to some questions: Is his definition contrary to the conventional definitions of conditioning? That is, in Pavlov's classical study was only the salivary response occurring in the complete absence of other concurrent responses? When a PDR does occur, how can

one be absolutely sure that no other generalized autonomic responses are occurring?

These two studies direct our attention to some important questions. Is it possible that the conditioned PDR (in Gerall's sense of conditioning) is based exclusively upon mediational mechanisms; i.e., cues resulting from the general fear reaction are conditioned to the PDR. Studies designed to investigate the CS-UCS interval should be able to answer this question. It is interesting to note that Young used a .6 sec. CS-UCS interval while Gerall *et al.* used a 1.5 sec. interval. Successful attempts (68, 76, 78) at pupillary conditioning with shock as the UCS all involved relatively lengthy CS-UCS intervals.

Gerall *et al.* interpreted their results within the confines of the reinforcement-contiguity controversy, judging them to be inconsistent with the assumption of some two-factor theories that reinforcement has "no effect upon the classical conditioning of responses innervated by the autonomic nervous system." Again one may ask whether this controversial question of yesteryear can be fruitfully revitalized without being subjected to marked linguistic modification?

Earl (48) offers quaint evidence against the reinforcement principle. He trained hungry mice to dig through nine pounds of sand to get food. During extinction, a hungry and food-satiated group failed to show any appreciable decline in their sand-digging behavior.

It is quite obvious that psychologists have still to discover many new kinds of reinforcement. Chapman & Levy (35) demonstrated with rats that a novel stimulus by itself can serve as a reinforcer. Berlyne & Slater (11) show that exploratory behavior of rats is greater with novel stimuli than familiar ones. They also show that a spacious goal box is more reinforcing than a simple blind alley and that complex stimuli are preferred to a simple stimulus situation. Studies like these raise two problems: How do these reinforcing stimuli operate, and how did they become reinforcing? The first question is concerned with the mechanisms underlying reinforcement, while the second is directed at discovering whether primary or learned drives underlie these reinforcing agents. Too often this question is answered by "negative evidence." That is, a reinforcing agent is said to be mediated by a learned drive if no one has discovered its physiological basis. Similarly a reinforcing agent is said to be mediated by a primary drive if no one has discovered how the drive could possibly be learned. It would be much better if the motivational basis of reinforcing agents is demonstrated in a more positive manner.

Smith & Duffy (170) have reported evidence which is interpreted to suggest that both need-reduction and sensory satisfaction can serve as reinforcers. Hungry rats learned more rapidly than satiated rats to choose the side of a T-maze containing a large amount (4 cc.) of a sugar solution when the other side contained the same amount of water. Both hungry and satiated rats learned at the same rate to prefer a small amount (.1 cc.) of a

sugar solution to a small amount of water. It was argued that the small reward groups had "too small an amount of sugar solution to provide any need reduction and that the learning in these groups is based on sensory satisfaction alone, while both factors operated, in the case of the 4 cc. groups, to favor the hungry group."

This interpretation may be questioned on two accounts. First, the mechanisms of need reduction and sensory satisfaction are not clearly independent. Miller (124) has shown that hunger can be regulated in at least two locations: mouth-throat and stomach-intestine. Perhaps physiological research will suggest, if the brain stimulation work (136) has not already done so, a common physiological base for the need reduction and sensory satisfaction concepts as used by Smith & Duffy. Secondly, Smith & Duffy's results are complicated by the use of water on the "nonreinforced" arm; water may have been an irrelevant reward whose influence depended on the degree of relevant privation.

In closing our section on reinforcement, attention should be directed to the interesting work of Marx (115, 116) and Marx & Goldbeck (117), who found evidence in favor of an after-gradient of a Thorndikian spread-of-effect when guessing-sequence and probability-bias factors were absent.

Amount of reinforcement.—Pereboom (142) failed to obtain the Crespi effect when testing the validity of Hull's Theorem 30. In spite of this he offers a new interpretation of the Crespi effect in another paper (143). He assumes that exploratory tendencies are extinguished more completely under a small reward than under a large reward. Thus, when a reward is shifted downward, Ss will display more exploratory behavior than the continuously small rewarded group. Consequently a depression effect will be exhibited. In contrast, Ss who experienced a shift upward should exhibit an elation effect because their exploratory behavior would have been effectively extinguished during the time they were receiving a small reward. Obviously what is needed is some direct measure of exploration in order to evaluate Pereboom's hypothesis. Metzger, Cotton & Lewis (121) also failed to find an elation or depression effect when the magnitude of reinforcement was shifted.

Wike & Barrientos (194) report rats preferred the arm of a T-maze that contained a drinking tube with a small opening to the arm that contained a tube with a large opening when the quantity of reward was held constant. The smaller tube produced greater drinking time and possibly more vigorous consummatory behavior.

Drive and habit.—The relationship between motivation and learning is far from clear. Part of the problem is the shroud of mystery and confusion that still covers much of the concept of motivation. Perhaps this shroud will lift when a more analytical approach (e.g., 124) is taken.

Lewis & Cotton (111) investigated the ticklish problem of whether drive level influences habit growth in a runway study. The acquisition drive had no effect on the over-all 30 trial extinction score. There was, however,

evidence of a significant effect of the acquisition drive during the first half of the extinction series. It was concluded ". . . that drive does affect habit strength, although the effect seems to be a relatively weak one." But even this conclusion is too strong. It is possible, as the authors themselves point out, that the Ss may have learned different speeds of running under the different drive levels. Instead of doing violence to the assumption that acquisition drive does not affect habit strength, the experiment of Lewis & Cotton highlights the difficulties of testing the independence of drive and learning in a runway or selective learning situation. Unless an experimental situation is used in which the response is the same and the delay of reinforcement is constant regardless of the level of the acquisition drive, any result showing the influence of acquisition drive may not be attributed directly to the level of drive itself but instead indirectly to a condition the drive produces (e.g., different responses). One other point: The question of the relationship between acquisition drive level and habit strength must be considered separately for appetitional and aversive drives.

Davis (38) showed how a shift in drive level can influence a panel-pushing response. His findings suggest that the performance exhibited under a high drive is not eradicated by a shift to a low drive. Such was not the case for a shift from a low drive to a high drive. He offers three interesting hypotheses to explain these results: (a) a residual motivation remains from the high drive, (b) the S learns to generate the high drive stimulus, and (c) different responses were being learned under high drive.

The role of drive level in selective learning has been investigated in a number of studies. Miles (122) divided human subjects into a high and low drive group in terms of their scores on a multiple choice thematic apperception test. In general he found high drive Ss to have larger proactive and retroactive losses in a proactive and retroactive inhibition experimental design involving a complex perceptual motor skill. These results were interpreted to be consistent with Spence's assumption (175) that a high drive should interfere with performance when an incorrect habit is dominant. Armus (4) tested this latter assumption in a T-maze study involving reversal learning. His results were suggestive but not significant.

Runquist, Spence & Stubbs (158) investigated a differential eyelid conditioning situation in which the intensity of the UCS was varied to produce a high and low drive group. The results obtained were consistent with the assumption of a multiplicative interaction between drive and habit, as well as being in general agreement with studies involving high and low anxious Ss (e.g., 183). Palermo (141) also reports findings which are in general consistent with Spence's interpretation (175) of the role of drive level in selective learning.

The Yerkes-Dodson hypothesis (sometimes referred to as a "law") (202), which states essentially that the optimum motivation for a learning task decreases with increasing difficulty of the task, seems to be entering a new phase of development. Broadhurst (26) obtained data consistent with

the hypothesis in an underwater discrimination situation. The level of motivation was measured, in a novel manner, by the amount of air deprivation produced by detaining Ss for different lengths of time under water. Brush (31) also reports evidence related to the Yerkes-Dodson hypothesis. He investigated the effects of different levels of shock intensity on avoidance learning. The results in general suggested that an inverted U-shaped function exists between performance and shock intensity. Brush interpreted his results in terms of the two-process theory of avoidance learning of Solomon & Wynne (171).

The Yerkes-Dodson hypothesis is related to the arousal or activation formulations of Duffy (46) and Malmö (114). They both assume that differences in activation are accompanied by differences in performance, this relationship being represented by an inverted U-shaped curve. Stennett (181) reports confirming evidence. But his study suffers from two major defects. There was much too much *ad hoc* selection of response measures. In addition, the logic surrounding the theoretical analysis of his data is open to question. Stennett found performance to be a function of the instructions he presented to the Ss. He also assumes that arousal is a function of the instructions. Why should he then conclude that the performance is a function of arousal and not vice versa?

A more direct manipulation of the theoretical concept of arousal is badly needed if the activation hypothesis is to be tested adequately. In addition the selection of the learning task should be given as much consideration as the selection of various physiological response measures. Stennett used an auditory tracking task. A simpler learning task might have served his needs more adequately. These criticisms should not direct our attention away from the importance of the hypothesis Stennett sought to test. Stennett has made one of the first steps that might ultimately be shown to be a step in the right direction.

Just as the Yerkes-Dodson hypothesis is related to the arousal hypothesis, so is the arousal hypothesis related to Spence's motivational theory (175): "The basic mechanism determining the level of drive (D) in aversive forms of stimulation is an internal emotional state or response of the organism (r_e).\" Are the arousal hypothesis and Spence's aversive drive theory (176) heading towards a common meeting place? They now differ in that the former postulates an inverted U-shaped curve between levels of arousal and performance, while the latter assumes a rising monotonic relationship, i.e., the stronger the drive the higher the response tendency. But this difference may prove more apparent than real. The change in the direction of the hypothesized performance curve as arousal increases towards its upper limits may be due not to high arousal *per se* but instead to secondary interfering effects high arousal produces. In line with this point, it should be noted that Spence's theory predicts that a high drive can retard performance in certain selective learning situations.

Perhaps the basic theoretical principle is that increasing amounts of

drive or arousal, or both, increase their "energizing" or "multiplicative" influence on habits. The study reported by Fuster (62) is relevant here. He initially trained monkeys to discriminate between two objects. The discrimination problem was then presented tachistoscopically. Discrimination was found to be facilitated by stimulation of the reticular activating system. Thus arousal in this case acted in a manner similar to an increased drive. The interpretation of this experiment brings up an interesting and important question for neobehavioristic theory. How do drives energize behavior? It has been assumed that drives energize behavior by acting on the S-R association. But the intensity of learned drives (1, 123, 175) is intimately related to the amplitude of certain responses. In Fuster's study the evidence suggests that the increase in performance was mediated by improved stimulus reception initiated by the stimulation of the reticular activating system. Perhaps drives can energize behavior in a number of ways: by lowering stimulus thresholds, by facilitating response evocation, and by activating the association itself.

DISCRIMINATION AND GENERALIZATION

All learning results from learning to discriminate. This is why theories of discrimination are considered to be so important. Up to now theories of discrimination learning have been limited in scope. The emphasis has largely been with the discrimination process itself, rather than how discrimination learning operates in a wide variety of situations. This past year has witnessed a greater concern with the second question, although the first question is still of primary importance (see discussion of transposition on pages 76-77).

Discrimination learning.—Gynther (73) utilizes Spence's (172) theory of discrimination learning to predict a substantial number of facts in a differential eyelid conditioning situation. The fit between the obtained facts and theory is most impressive. Davis (39), Ginsburg (67), and Leary (110) also report data that are consistent with various aspects of Spence's interpretation of discrimination learning (172, 174).

Babb (7) simply but effectively shows how differentially discriminable components of a stimulus compound become associated with an instrumental response. Babb trained three groups of rats to respond positively to a brightness cue (white or black) or the presence or absence of chains, or a combination of both stimulus elements (e.g., black plus chains). The slowest rate of learning occurred to the chain-no-chain discrimination, with no significant difference occurring between the other two discrimination problems. Results of the transfer test showed that learning occurred to both elements of the stimulus compound. It was also shown that experience with the cues in the compound was less beneficial to the less discriminable cue (chains-no-chains) than to the more discriminable cue. These results supported Wickens' (193) "continuity" interpretation of some of Lawrence's results (108, 109).

Hanson (75), using a Skinnerian technique, reports data favorable to Spence's theory of discrimination learning. After discrimination learning Ss tend to respond at a higher rate to stimuli adjacent to the positive stimulus, and at a lower rate adjacent to the negative stimulus. The highest rate of responding was not given to the positive stimulus but rather to the next adjacent one, suggesting the operation of generalization of inhibition.

Mediational hypothesis.—In order to account for more complex behavioral phenomena S-R psychologists have found it necessary to theorize about hypothetical stimulus and response events. A number of mediational hypotheses have been formulated to account for a variety of data. All of these formulations are in essential agreement in that it is assumed the S generates hypothetical responses that produce hypothetical cues which are capable of becoming conditioned to overt responses.

In some cases the response-produced cues enhance a discrimination. Goss & Greenfield (70) investigated the learning of discriminative perceptual-motor responses involving the pushing of a lever in a different direction to four different light intensities. Learning to make different responses (primarily verbal) to these four light intensities prior to the discrimination training facilitated the learning of the perceptual-motor task itself. In short, developing distinctive response-produced cues facilitated the discrimination among four physically different light intensities. Braun & Bendig (23, 24) also report evidence of favorable effects of verbal cues on perceptual-motor learning, while Hoffield (85) demonstrates both positive and negative transfer of verbal pretraining on a mirror drawing task.

Kendler & Karasik (101) also show how distinctive response-produced cues facilitate discrimination training. Ss were required to select four of eight words that belong to a common concept. It was predicted, and the prediction was confirmed, that the speed of concept formation would vary directly with the ability of the irrelevant words (the words that did not belong to the concept) to evoke implicit responses that are distinctively different from those required for the relevant words.

In other cases response-produced cues enable an organism to respond in a common manner to disparate observable stimulus events. Coppock (37) developed some interesting procedural variations in a sensory preconditioning study designed to test a mediational interpretation (137) of that phenomenon. This writer believes the results Coppock reports are in complete accord with the mediational hypothesis, even though Coppock entertains some reservations. Perhaps some of these interpretive problems can be resolved by experimental attempts to record the hypothesized mediating responses. The mediational S-R approach was put a step forward by the excellent work of Staats & Staats (177), who demonstrated that the concept of meaning may be treated similarly to a conventional conditioned response. Staats & Staats paired different words possessing a common meaning component with the same nonsense syllable. The syllables were found to have acquired the predicted semantic loading. In addition, the mediational ap-

proach was enhanced by Phillips' study (147), which presented a clear demonstration of directly induced secondary (mediated) generalization. Norcross & Spiker (134) demonstrated that mediated associations can lead to positive or negative transfer in the paired associate learning of young children. Jeffrey & Kaplan (91) report some evidence of semantic generalization with experimentally induced associations. However, not all of their predictions were confirmed.

Psychology in general and the mediational hypothesis in particular should profit from Osgood, Suci & Tannenbaum's recent work (138) on the semantic differential method. They have combined the forces of the mediational hypothesis with factor analysis to produce objective measures of meaning that should encourage tough-minded research in many areas of human behavior that heretofore lacked adequate means of coping with the problem of meaning.

Intermittent reinforcement extinction effect.—The explanatory problem associated with the intermittent reinforcement extinction effect (IREE) still controls the attention of many S-R psychologists. Although a number of different S-R explanations have been offered, none has become widely accepted.

Freides (61) has attempted to approach the IREE by dividing a runway apparatus into two discrete sections: a runway and goal box. After analyzing the large amount of data his complicated experimental design produced, Freides concludes that a proper analysis of the IREE requires "a detailed analysis of the responses elicited by each 'type' of stimulus and a careful delineation of both the excitatory qualities of that stimulus and its inhibitory susceptibilities." He also concludes, and mistakenly so, that his results are in conflict with other current S-R interpretations (102, 107, 192, 197) of the IREE.

Denny, Wells & Maatsch (41) also attempt to break down the IREE into component parts. They tried to accomplish this not by any novel apparatus design, but instead by observing different sections of the instrumental response chain. They found that intermittently reinforced Ss made more bar pressing responses during extinction but their approach responses to the food tray, although intermittently reinforced, did not show any greater resistance to extinction than did continuously reinforced Ss. This result was obtained with a fixed-ratio schedule with a click and reward occurring every fifth trial during acquisition and the click alone every fifth extinction trial. The results are interpreted to "support a discrimination analysis of the effects of partial reinforcement in extinction."

The use of delayed reinforcement may provide some interesting insights into the IREE. Wike & McNamara (196) trained three groups of rats to traverse a runway under conditions of 25, 50, and 75 per cent intermittently delayed reinforcement (30 sec. in a delay chamber). The results surprisingly suggest that the lower the percentage of reinforcement the faster the running speed at the termination of training. During extinction the 25 per cent

group extinguished most rapidly, with no difference existing between the other two groups. This kind of experiment should be followed up. A factorial design involving various amounts of delay and percentages of reinforcement, as well as measures of the instrumental response and goal box responses, might provide facts that could test the integrative powers of various theories of the IREE.

Of possible interest to the theoretical analysis of the IREE are the data reported by Berkun (10). He found that the recovery from an approach-avoidance conflict in a straight alley was more rapid when the approach response had been intermittently, rather than continuously, reinforced. Berkun suggests that perhaps the intermittent reinforcement of an approach habit increases its resistance to the disrupting effects of shock in the same way that an intermittent reinforcement schedule increases the resistance of a habit to the disrupting effects of extinction. Are the psychological consequences of shock similar to nonreinforcement, or is Berkun merely making a superficial analogy? Is it not possible that in Berkun's experiment the cues at the beginning of the straight alley are more strongly conditioned to behavior occurring in the end box for the continuously reinforced Ss, thus enabling them to develop stronger anticipatory avoidance responses to the end box following shock? The problem Berkun raises is an interesting one. If Berkun is correct, then embarrassing problems will be raised for those formulations that interpret the IREE in terms of the similarity between responses on nonreinforced trials during acquisition and extinction.

Stimulus intensity dynamism.—In a well-planned study, Nygaard (135) tested the relative merits of Hull's (88) stimulus intensity dynamism (V) postulate and Perkins' (145) and Logan's (113) formulation that the stimulus intensity effect is dependent upon the relation between the intensities of the cue stimulus and of the contextual stimuli. The results favored the relative interpretation which would also be consistent with Riley's formulation (153). This relative interpretation raises problems for the interpretation of the results of Jerome *et al.* (92) and Henderson (80), who fail to report the nature, if any, of the external illumination. Jerome *et al.* found that the mean number of crossings to a dark box was an increasing function of the start box light intensity. This result was not due to the failure of weaker intensities to activate performance but to their failure to maintain the rate of response they initially activated. Henderson failed to find any relation between stimulus intensity and the amount of secondary reinforcement. It is important for studies of this sort to have careful control of contextual illumination.

RETENTION AND TRANSFER

S-R functionalists have specialized in problems of human retention and transfer. They are the modern descendants of Ebbinghaus who have learned all about operationism and experimental design. Their contributions, how-

ever, are not as integrated as more broadly systematic viewpoints, for the simple reason that S-R functionalists do not like their theory to forge too far ahead of their facts, an attitude that is considered by some to be an expression of intellectual meekness, while to others it denotes intellectual fortitude. Because of the relative independence of many of these studies, the task of summarizing them was most difficult. Space limitations worked the greatest hardship in this section.

Retention.—By demonstrating that distributed practice facilitates paired associate learning but has no influence on retention Underwood & Richardson (188) show that retention is not simply a function of learning.

Runquist (157) approaches the traditional problem of rote learning and forgetting in a somewhat untraditional way. Instead of dealing with recall of a list of words, Runquist makes an attempt to analyze the retention of the individual items within the list as a function of their strength. Several of his results are most surprising and will, if attended to, suggest a number of important studies designed not only to understand retention but also the consequences of conventional methodological procedures. Runquist suggests that the "... differentiation between the two response systems may be the most important factor in forgetting due to interfering learning."

Steinberg & Summerfield (180) and Summerfield & Steinberg (182) provide a neat confirmation of the interference theory of forgetting. They first (180) demonstrate that the administration of the central depressant drug nitrous oxide impaired the formation of associations. They then showed (182) that the administration of nitrous oxide immediately after learning reduced forgetting, presumably by reducing interfering learning. But like a voice from the wilderness Brown (28) argues for a more sympathetic consideration of a decay process in the forgetting of items after the delay of several seconds, during which time he feels interference is not operating.

And finally Dowling & Braun (45) report that meaningfulness was related to retention when retention was measured by recall, but not when it was measured by reconstruction and relearning. There is obviously still much to be done to clarify the relationship between retention and meaningfulness, which seems to be becoming increasingly complex.

Reminiscence.—Riley (152) reports a definite demonstration of reminiscence while Underwood (187) fails to find it. Underwood concludes that if "... a performance-depressing inhibition is postulated to account for poorer performance under massed than under distributed practice, this inhibition cannot be assigned rapidly dissipating properties."

Transfer of training.—This topic is so pervasive it is questionable whether it should be isolated as a separate category. Duncan (47) asks a traditional transfer of training question about how much of the increasing positive transfer called learning to learn is due to S experiencing a variety of tasks and how much is due to sheer practice. He finds, in a factorial design that separates practice from variety, that transfer increased both as

variety of tasks and amount of training increased. Duncan believes that the advantage of varied training on transfer is that it trains S to make receptor orienting acts to various stimulus elements.

Murdock (129) reports evidence that backward associations (i.e., during the learning of an A-B association in paired-associate learning a B-A association is also formed) transfer. It would be interesting to see how the formation and transfer of backward associations develop with age. Such evidence would throw light on the changing structure of language as verbal behavior develops.

AN OVERVIEW OF S-R FUNCTIONALISM AND NEOBEHAVIORISM

S-R functionalism is bound to survive and thrive. The problems it faces are neither large nor numerous. As long as psychologists are interested in limited problems S-R functionalism will be an active orientation. It should be noted, however, that the theoretical conservatism of S-R functionalism does not in practice force one to adopt a limited formulation. Woodworth (199) recently has tried to integrate a wide variety of data within a formulation that can be appropriately assigned to S-R functionalism. The question is whether such an attempt was made in spite of or as a result of the tradition of S-R functionalism. It will be interesting to see whether other S-R functionalists dare follow Woodworth's example.

What does the future hold in store for neobehaviorism? The answer to this question lies in the ability of neobehaviorism to solve some of its pressing theoretical questions, particularly those related to its deductive rigor. When inconsistent results are obtained, it should be possible to know which theoretical assumption is at fault. This, however, would depend on the assumptions within the theoretical system being independent. Such is not always the case in neobehavioristic theory. It has already been noted that the sharp theoretical distinction between habit and drive is often blurred on the experimental level. This condition seriously impairs the deductive rigor of neobehaviorism. Perhaps neobehaviorists should adopt a new canon: Differences in performance should be attributed to motivation only if they cannot be assigned to learning. In any case, there is much need for a clearer distinction of these two concepts on the operational level or, if that is impossible, a revision in their theoretical status.

The second critical problem facing neobehaviorists is the greater use of hypothetical stimulus and response elements (e.g., r_g-s_g) in their theorizing. These stimuli and responses are sometimes conceived as directly observable, other times potentially observable and still other times as purely hypothetical. In actual usage such concepts are inferred theoretical constructs, with an epistemological status more similar to concepts such as drive than to directly observable stimulus and response events. A full realization of this point should force the theorist to treat them with greater caution so that the number of assumptions they contain do not exceed the number of facts they seek to explain. The virtues that can ensue from the

proper use of hypothetical stimulus and response events are shown in Moltz's (126) treatment of latent extinction.

In rebuttal to criticisms, it can be argued that neobehaviorism offers a relatively ambitious theory that seeks to tie together diverse and scattered facts. Such a theoretical effort, at this stage of psychology, must be made at the expense of deductive rigor. With more information perhaps its deductive rigor will improve. And there are some signs (51) that it is doing just this. One is reminded, when surveying the theoretical attempts of the neobehaviorists, of Napoleon's comment on his military strategy: "*On s'engage, et puis—on voit*"—"You commit yourself, and then—you see." Does such a strategy inevitably lead to a Waterloo?

SKINNER

Skinner is an enigma. His attitudes, and those of many of his partisans toward facts, theories, statistics, and science itself are incongruous. But in spite of this his work, as well as that of his rapidly growing legions of adherents, is fascinating and creative, and has much of permanent value.

How does one evaluate the contributions of the Skinnerians? One can accept their framework of science and simply but enthusiastically list the facts discovered in the past year in more or less random order. Or one can attempt to evaluate the contributions of the Skinnerians within a framework of science that is alien to them. Both will be attempted, with the realization that there is little common ground between the philosophy of science underlying Skinner's system and that of other contemporary theoretical orientations.

Major works.—Skinner produced two major works (60, 168) during the year. He was the sole author of *Verbal Behavior*, while Ferster was his co-author in *Schedules of Reinforcement*. Never in the history of psychology has one person authored two such contrasting books. *Verbal Behavior* is practically void of facts and filled with speculation. *Schedules of Reinforcement*, on the other hand, is filled with facts and void of speculations. In spite of this apparent incongruity, both books carry the bold imprint of Skinner's system.

In *Verbal Behavior* Skinner superimposes his analysis of the conditioning behavior of animals onto the verbal behavior of humans. Feeling his experimental analysis of behavior is "surprisingly free of species restrictions," Skinner shows how it is possible to analyze verbal behavior within his radical (or reactionary?) positivistic S-R analysis.

Whether he fails or succeeds depends on one's expectations of a book of this sort. The book is filled with brilliant analyses of a wide variety of verbal responses. Some critics might note a similarity in quality between Skinner's "convincing" analysis of language behavior and Freud's equally "convincing" analysis of clinical case histories. This might appear to be a shocking comparison (to both Freudians and Skinnerians), but it should be realized that Skinner's analysis of verbal behavior is not automatically vali-

dated because it springs from the hard data of the Skinner box studies. The validity of Skinner's analysis must be judged against the facts of verbal behavior—of which there are relatively few. Skinner, in lieu of facts, is forced to fall back upon everyday observations and examples from literature, philosophy, and some psychology. The ultimate worth of *Verbal Behavior* will be determined by the positive orientation it provides to future researchers in the field of verbal behavior.

One point of contrast between Skinner's analysis of verbal behavior and those of theoretically oriented S-R psychologists (e.g., 44, 137) should be noted. Skinner believes it is possible to describe verbal behavior without recourse to any "hypothetical explanatory entities." Theoretically oriented S-R psychologists, on the other hand, deal with verbal behavior by postulating hypothetical stimulus and response events defined in terms of experimental manipulations. The question is whether these two approaches are as different as they seem to be or as Skinner states them to be. In his analysis of thinking Skinner discusses covert verbal behavior. But what is covert verbal behavior? It is not directly observable. We infer it from introspective reports. The concept is hypothetical, and in this sense is similar to the mediational assumption of some S-R theorists. It might be asked whether Skinner does not actually postulate theoretical constructs and then refuse to deal with them.

Schedules of Reinforcement (60) represents the other side of the two-headed coin known as Skinner's system (one side—narrowly factual and atheoretical; the other side—highly speculative and practically afactual). In this book consisting of 739 pages, 921 figures, and 13 references, an enormous amount of data is reported about how the rate of a pigeon's pecking response is related to its schedule of reinforcement. A large number of schedules are investigated and it is shown how subtle changes in the schedule can produce marked changes in an animal's behavior. Since this book fails to exhaust the practically infinite number of schedules, it can be reasonably expected that the kind of work reported in this book will be continued *ad infinitum* (or *ad nauseam*).

Again the expectation of the reader will determine the evaluation of this book. To those psychologists who are starved for facts this book will satisfy their hunger for many a moon. To those psychologists who are interested in controlling behavior, this book will offer both exhilaration and example. To those psychologists who are interested in quantitatively expressed empirical laws, this book will be a source of frustration. But the book offers many demonstrations that will suggest a source for potentially important empirical laws. The behavior theorist will be thwarted by this book. If he can control his temper he will, however, realize that many interesting theoretical problems remain hidden and unanalyzed within its covers. The theoretical importance of hypothetical stimuli (response produced stimuli) is just as apparent in *Schedules of Reinforcement* as in *Verbal Behavior*. In short, *Schedules of Reinforcement* will sicken the statistician, puzzle the philosopher of science, and challenge the non-Skinnerian psychologist.

One other major event occurred within the Skinnerian system. The new *Journal of Experimental Analysis of Behavior* was born. One cannot avoid commenting upon the increase in the number of new journals and the impossible task that is imposed upon the scholar who desires to keep abreast of recent developments. Perhaps it was inevitable that the Skinnerians would start their own journal because of the combined effect of their unconventional methodological attitudes and the traditional orientations of the major journals. It would be unfortunate, however, if their new journal increases their insularity. The Skinnerians have much to give to psychology—and much to receive.

Experimental contributions.—One can detect in viewing the literature two ways of operating within the Skinnerian framework. The work of Sidman (who gets this year's prize for the highest publication response rate) and several associates, notably Boren, represents an extremely positivistic interest in individual facts with a disregard for their systematic status or implication. In one paper (164) Sidman, Herrnstein & Conrad write: "It has been our experience that an experimental design in which each step depends on current data is more economical and productive than a preconceived and fixed plan of experimentation." This "option-play" (or "playing-by-ear") type of research generates a mass of data that is most difficult to analyze systematically. Each experimental article contains many more variables than subjects. The knowledge obtained, as well as the interpretation offered, is that some variables, in conjunction with other variables, result in changes in behavior. At the other extreme is some work of Hearst (79), Herrnstein & Morse (82) and Kelleher (95, 96). These researchers operate within a Skinnerian framework. But they are teased, if not seduced, by quasi-theoretical questions. Their findings force them to ask questions which can be answered only by more traditional experimental designs. It will be interesting to observe whether they will answer the questions that they themselves raise.

Sidman and a number of associates have examined the influence of several variables on the maintenance of avoidance behavior. Sidman & Boren (162) systematically varied the interval by which the instrumental response postponed a shock. Each time a shock occurred the length of the interval was varied among a predetermined set of five intervals (4, 10, 15, 20 sec. and one of the following four maxima: 20, 25, 40, and 80 sec.). In a setup such as this the most effective behavior would be to space responses just short of the minimum interval in the series, so that shock would never occur. The behavior seems to adjust itself somewhere between this ideal extreme and the opposite extreme which resulted from the maximum interval. As the maximum interval increased, the average response rate decreased and there was a greater frequency of longer interresponse intervals, but the most frequent interresponse interval remained constant at an intermediate value.

The authors attribute these two findings to two variables: the maximum

interval controls the relative frequency of long interresponse intervals; the minimum intervals control the peak of the frequency distribution of interresponse times. Thus the conclusion is offered that the avoidance behavior observed was governed by some resultant condition produced by all the intervals in the series. It is interesting to note that the temporal course of behavior in each session showed cyclical variation. This suggests that the Ss learned to respond more rapidly than the minimal 4 sec. interval. When this occurs, however, the S avoids a shock. As a result the shock avoidance responses weaken, with a consequent increasing of the interresponse time. Shocks are then received and the rate of responding increases. Then this cycle repeats itself.

In another study Boren & Sidman (17) trained Ss to press a lever in order to postpone shock. A shock was administered whenever 20 sec. elapsed without the S responding. Then an intermittent procedure was instituted in which shock was delivered only a predetermined percentage of times after 20 sec. elapsed. The rate of responding remained relatively constant if the percentage of shocks given varied from 30 per cent to 100 per cent. Below 30 per cent the rate dropped sharply and discontinuously. The authors account for this behavior by the combined effects of the ongoing behavior and the schedule of shocks.

In another study (164) it was found that occasional "free" shocks increased the rate of free avoidance responding of monkeys. Stimuli that had been coupled with free shock retarded extinction. These facts were interpreted in terms of the "superstitious" avoidance behavior (which is inferred but not observed) maintained by spurious response-shock contingencies.

Sidman & Boren (161) in another study may be accused by unsympathetic critics of discovering the obvious. They compared the behavior of two groups of rats who were given a warning signal (light) that preceded a brief shock. For both groups, pressing the bar postponed the shock and the warning signal that preceded it by 5 sec. Once the light went on the avoidance group was able to postpone the shock by pressing the bar. In contrast, the anxiety group was forced to take the shock if the light went on. It was found that the avoidance group pressed the bar at a higher rate during the time the warning signal was on.

The most interesting, and probably the most important, of these studies was one reported by Sidman & Boren (163). They asked whether anticipation of pain is worse than pain itself. Their findings suggest a positive answer; the rate of responding of rats is higher when the rat takes the shock and terminates the warning signal than when the rat postpones the shock and prolongs the warning signal. The question of why a cue associated with punishment can become more aversive than the shock from which it developed its aversive properties is an important theoretical question for all psychologists to consider. Can the answer to such a question

come from the casual kind of experimental design that Sidman approves?

The study of Herrnstein & Morse (82) differs subtly, although importantly, from the studies just described. They trained pigeons by a differential reinforcement procedure to make a key-pecking response at a low rate. A neutral stimulus was then aperiodically presented and followed by a food reinforcement regardless of the Ss' behavior. This produced a large increase in the rate of the operant response. The authors rule out the possibility that this increase results merely from "... accidental temporal correlation of the operant and the response-independent reinforcement." The authors entertain questions raised by their results and express an interest in planning systematic research and in obtaining empirical relationships. Both this study and the previous one (164) provide the kind of data that the incentive motivation theory assumption of Spence (175) seeks to integrate.

Kelleher (95, 96) is interested in two questions: Can conditioned (secondary) reinforcement presented intermittently maintain behavior? If primary reinforcement is delayed, how much is the effectiveness of the conditioned reinforcers decreased? In one study (95), when two chimpanzees were under a 5 min. fixed interval schedule of conditioned reinforcement, lever pressing was sustained when Ss were required to earn groups of poker chips (from 1 to 6) before exchange for food was possible. Extinction occurred when 8 poker chips were required for exchange or when poker chips could only be exchanged hourly. In another study (96) a multiple schedule of conditioned reinforcement, comprised of randomly programmed sequences of 5 min. fixed-intervals and 20 response fixed-ratios, was used. Lever pressing behavior was "... sustained by conditioned reinforcers at exchange delays ranging up to about 200 min." Why, asks Kelleher, is the multiple schedule so much more effective than the 5 min. fixed-interval schedule? He suggests some answers and concludes "further research will be required to determine the existence and nature of ... interactions in multiple schedules of conditioned reinforcement." If Kelleher means what he says and does what he suggests, the kind of research that will result will move in the direction of more traditional research designs and data treatment techniques than is customarily practiced by Skinnerians.

A somewhat similar attitude is expressed by Bijou (13, 14). He observed the behavior of four four-year-old children during extinction following training on fixed-interval schedules of reinforcement of 20, 30 and 60 sec. Bijou (14) concludes, "... compared with infrahumans, children show tremendous variability in the number of responses during extinction for a given schedule of reinforcement." He attributes this to the ability of children to generate their own stimuli, which in his experimental setup is not yet under the control of E. Bijou assumes that, "If the response-produced stimuli have positive discriminative or reinforcing properties ... the response rate and number increase. ... If they have discriminative

functions leading to competitive or aversive behavior . . . the response rate and number decrease." Bijou concludes that a series of systematic experiments is needed to test his hypothesis.

Hearst (79) poses a modest problem that no doubt appears shockingly theoretical to some Skinnerians. He attempted to show how "interval" and "ratio" behavior can be integrated in terms of a single set of temporally defined variables. A schedule of reinforcement was arranged so that only one reinforcement was available for a limited period every 30 sec. As the length of this limited period decreased response rates and responses per reinforcement in general increased. Thus variation in the duration of this limited period "led to a change from interval-like behavior to behavior resembling that seen under ratio schedules."

An article by Skinner & Morse (169) is interesting if only to reflect the growing interest among Skinnerians to behavior in the experimental situation other than the instrumental response. They sought to discover how rats (two to be exact) behaved when they were not pressing a bar during a 5 min. fixed-interval schedule. A running wheel was available to the rats. They could not, however, press the bar and run simultaneously. When the Ss pressed the bar at a high rate, the running "was suppressed." When the bar pressing rate was low, running occurred. "For a substantial period of time after each reinforcement, the lever is not pressed and running does not occur." This period of time ". . . is much longer than the time required to eat the pellet of food." Keller & Schoenfeld (98) note a similarity between the fixed-interval schedule and Pavlovian delayed trace conditioning. Perhaps this inactivity following a reinforcement that Skinner & Morse report is similar to the inhibition of delay noted by Pavlov during the early portions of a delayed trace conditioned response. This possibility could be tested. If internal inhibition was operating in Skinner & Morse's study, then increasing the fixed-interval would also increase the period of inactivity following reinforcement. Or a disinhibitory stimulus, during this period of inactivity, should reinstate one of the two responses. It would be interesting and somewhat amusing if the results of these suggested studies supported an internal inhibition interpretation of Skinner & Morse's finding. Skinner might be forced to catch up to the theoretical position of Pavlov.

New techniques and novel facts.—As is their custom, the Skinnerians have developed a number of new, fascinating experimental techniques and interesting results. Brady *et al.* (19) have used operant conditioning techniques to produce gastroduodenal ulcers in monkeys. Morse & Skinner (127) report another type of superstitious behavior in the pigeon. They found that a "stimulus present when a response is reinforced may acquire discriminative control over the response even though its presence at reinforcement is adventitious." Lindsley (112) demonstrated how operant behavior could be used to study behavior during sleep. He investigated how amount of sleep deprivation and effects of drugs influenced the operant level of a thumb moving response that reduced the intensity of a tone.

Blough (16) has reported an ingenious operant-conditioning procedure designed to obtain psychophysical visual thresholds of the pigeon.

Ferster (58) describes a procedure for establishing two different response sequences in the same animal. Different rates of pressing two keys by a chimpanzee were established by using different schedules of reinforcement for each key. A high rate of pressing the right key was mediated by using a fixed-ratio schedule, while a low rate was established for the other key with a fixed-interval schedule. The performance of the animal on both keys was "similar to those that would develop singly without interference from the behavior on another key." Falk (57) was able to teach a chimpanzee a discrimination by allowing the S to groom the E (whose behavior was being reinforced?). Kelleher (97) has reported on an experimental technique that can serve as an "operational index of 'attention.'" And Herrnstein & Morse (83) have investigated one of many possible conjunctive fixed-interval, fixed-ratio schedules.

Butler (34) investigated the visual exploratory motivation of Rhesus monkeys in an operant conditioning situation. The Ss were deprived of a varied visual experience for 0, 2, 4, and 8 hr. They were then placed on a variable-interval reinforcement schedule in which reinforcement consisted of a 12 sec. view of a monkey colony outside the test cage. "The results showed that the number of responses to visual incentives approached a maximum after 4 hr. of deprivation. . . . These data provide another demonstration of the similarities between behavior based on a proposed curiosity motive and behavior based on biological drives."

Brady *et al.* (18) found that hunger and thirst affect behavior maintained by brain stimulation. Lever pressing rates with both cats and rats increased systematically as a function of increasing deprivation intervals. Of interest is the fact that with brain stimulation, continuous reinforcement produced a higher rate of responding than did intermittent reinforcement. Ferster (59) found that the withdrawal of a positive conditioned reinforcer had the functional properties of punishment, i.e., it suppressed the rate of responding.

Concluding remarks.—Where does the Skinnerian formulation go from here? At present the Skinnerians have been able to generate an enthusiasm and conviction among themselves resembling a quaint mixture of that found in a revolutionary party, a revival meeting, and a homecoming football gathering. Can this enthusiasm and conviction maintain itself on such a skimpy diet of theoretical notions? This writer thinks not, if for no other reason than that Skinner's extreme methodological position (159) will disintegrate in the face of additional facts and more complicated experimental situations. Dinsmoor (42) has designed a device which would measure variations in the characteristics of a single operant response. How would results obtained from such a device be incorporated meaningfully into the conventional Skinnerian cumulative curves? Some British psychologists (90, 186) have recorded many more kinds of response measures in an

operant conditioning situation than is the custom of their American counterparts. How are such data to be handled without a theoretical frame of reference?

Rumblings are even now being heard. If the extreme methodological position of Skinner is not being questioned by Skinnerians, we have noted signs that it is being ignored.

At present the empirical foundation of the Skinnerian orientation is not as solid as the Skinnerians would like to believe. Many of their facts are at best only suggestive. Instead of discovering empirical laws, the Skinnerians merely offer a demonstration of how the law operates. Their method, in this sense, resembles that of the Gestalt psychologist who offered numerous demonstrations of perceptual organization without satisfactorily stating the nature of the laws.

How can so many important experimental techniques and interesting findings spring from such an antitheoretical attitude? One contributing factor is the sheer genius of Skinner himself. This writer suspects that if it had been possible for Skinner to have applied his restless and brilliant intellect to the revival of structuralism, he would have been eminently successful. Another factor is the fascination many Skinnerians have with the idea of controlling behavior. This has led them directly to potent psychological variables. And finally the Skinnerians have indulged in covert theorizing even though they refuse to recognize it. There are other factors also. But it would be both foolish and naive to attribute the successes of the Skinnerians to their antitheoretical, antiquantitative and antiresearch design positions.

One concluding point. Skinner himself has always extolled the virtues of efficient research. He and his co-workers have been largely responsible for the design of automatic apparatuses and recording instruments. But is his research program as efficient as his experimental techniques? *Schedules of Reinforcement* was based on a research program that involved "approximately 70,000 recorded hours, during which the experimental organisms emitted approximately one-quarter of a billion responses." Was the payoff in knowledge commensurate?

STATISTICAL LEARNING THEORY

Statistical learning theory is very young if we date its birth from Estes' 1950 (50) article. To judge it fairly one must consider its infancy. But at the same time, in order to insure its healthy development, statistical learning theory must not be overprotected or overindulged.

The quantity of publication of statistical learning theory is increasing but still is relatively small when compared to the productivity of the neo-behaviorists and S-R functionalists. This condition, to some extent, can be attributed to the simple fact that there are not very many statistical learning theorists. Psychologists who received their doctoral degrees from the

few professors who practice the magical arts of stochastic models have not had much time to propagate the faith by training their own students.

The contributions in statistical learning theory of the past year can be classified into three general categories: (a) experimental tests of theory, (b) theoretical articles designed to integrate available data, and (c) the purely theoretical article which is practically unique to mathematical learning theory. In this type of article new relations between theoretical concepts are deduced, but no reference is made to any empirical findings.

Experimental tests of theory.—Statistical learning theory has been successful in predicting matching behavior. This is where the probability of a S making a particular response at asymptotic value equals the probability of that response being reinforced. Interest has continued in this problem. Researchers have attempted to discover whether, in more complicated probability learning situations than those initially used (56), a given choice response would stabilize at a point equal to the percentage of time it is being reinforced.

Estes & Straughan (56) applied the model successfully to the case in which a S was to guess which of two lights (E1 or E2) would go on. Anderson & Grant (3) have extended the Estes-Burke-Straughan (53, 56) model to cover light-guessing situations involving both the occurrence of single events (E1 or E2) and double events (both E1 and E2 or neither E1 nor E2). The model gave "fair but not perfect predictions of terminal response rates." Responses on consecutive trials were found to be dependent which could be interpreted in an *ad hoc* fashion as indicating that the samples of stimulus elements drawn were not independent. Group learning curves also did not reflect individual performances. Anderson & Grant conclude, "The partial success of the present model indicates more that the behavior under study is not as simple as the model assumed than the basic theory is incorrect."

Brand, Sakado & Woods (20) tested the Estes model in a contingent partial reinforcement situation in which the to-be-predicted event is a function of two probabilities: the probability of the E assigning the correct light (e.g., red instead of white) to the correct lever and the probability of the S pressing the correct lever. The results showed that the model significantly underestimated the empirical asymptotes. In defense of the model it should be noted that in applying the model it was necessary "for Ss to assume that the alternative response would have been correct" when making an incorrect response. It is likely that the instructions used to insure this set were not effective.

Estes *et al.* (54) provide an impressive extension of their statistical learning theory to data obtained in a probabilistic discrimination learning situation involving cues that are differentially correlated with reinforcing events at intermediate values between unity and zero. According to their theory the responses to the individual cues should proceed in a manner

similar to simple instrumental conditioning experiments with a random intermittent reinforcement schedule. Their experiment produced results consistent with their theoretical curves which had only one free parameter.

Estes & Johns (55) have also extended Estes' model to a modification of the conventional experiment on probability learning. S was given an ambiguous reinforcing stimulus after making his prediction of which of two uncertain events would occur. S was thereby forced to judge whether the event had in fact occurred. It was found that S was more likely to judge that a given event occurred if he predicted it. It was also found that asymptotic proportions of predictions matched closely the proportion of judgments, thus confirming, according to the authors, "a theory which assumes that S's response to a stimulus is a critical determiner of its reinforcing effects."

Brody (27) sought to extend Estes' model to an instrumental avoidance situation. The asymptotic response probabilities, as well as the forms of the performance curves corresponded fairly well with the predictions from the theory. One problem associated with measuring the extent of the agreement between the model and the data is that the particular model being tested has no rivals. It is easy to evaluate the ability of two different models [as did Gardner (64)] to fit the same set of data. The problem of evaluating the goodness of fit of one model by itself is a more difficult task. Audley (5) refers to the same problem in his paper in which he seeks to include response times within a statistical description of learning behavior.

Komorita (105) was not too successful in the application of statistical models to the prediction of asymptotic response probabilities. He used a situation in which Ss had to predict which one or which two of three lights would go on. Matching behavior was found in four of six situations. The discordant results suggested that the matching model may not be applicable when the probabilities of the stimuli do not sum to unity.

La Berge & Smith (106) report evidence for a selective instead of a random sampling of cues in a discrimination-learning situation. They found that when Ss were responding at or very near asymptotic values they were ignoring common background elements. The authors suggest that perhaps the common elements became "adapted" (148) during training or else that the common elements disappear from the sample because the "observing response" (200) becomes directed toward the differential elements.

All mathematical learning theories do not spring from the same source. Hansen (74) has applied a simple probability model, much like that used by Mendel in his classical work in heredity, to data collected in a so-called V-device which is somewhat more complex than a T-maze. His analysis, which includes reference to the phenomenal world as well as the physiology of his Ss (mice), in addition to stochastic processes, is interesting if only to give the reader an insight into some of the sociological factors that influence theory construction on the other side of the Atlantic.

Starting from a Hullian orientation, Birch (15) has suggested a model

to predict performance in a two-choice situation to a stimulus compound as a function of the component stimuli. He finds that his proposed combinational rule proves adequate in integrating some data. Noble (130 to 133) also is working toward a mathematical theory within a Hullian orientation. Overall & Brown (140) have formulated a cognitive-probability model that incorporates both a recency and a frequency principle.

Researchers have also been interested in the influence of a number of experimental variables on matching behavior. Matching behavior has been noted to be "irrational." In a noncontingency case (56) if the left light is correct in 75 per cent of the time and the right light 25 per cent of the time, the strategy that will maximize successful predictions is to guess on every trial that the left light will go on, while in matching behavior the left light is guessed on only 75 per cent of the time. One view is that the "irrationality" of matching behavior is due to the S's belief that there is a pattern operating that would allow him to be correct on every trial. By continuously searching for such a pattern the S "falls into" matching behavior. If this is so then changes in instructions should influence the behavior of the S.

Anderson & Grant (3) included variations in instructions after the basic information about operating procedures was conveyed to the S. No noticeable effect of differing instructions was observed and this finding was attributed to the stability of the matching behavior phenomenon. Another obvious possibility was that the instructions were ineffective in producing a different set.

This latter criticism may apply also to another study reported by Brand, Sakado & Woods (21). Different instructions failed to produce different results. This might have resulted from the Ss' failure to understand the term "random." Again it seems possible that the failure of different instructions to influence probability discrimination is simply due to the failure of the E to use instructions that were functionally different. One danger that statistical learning theorists have to guard against is to ignore the subtleties of experimental techniques while simultaneously paying attention to the subtleties of their formal system. It is interesting to note that in both studies (3, 21) just described, no reference is made to Wyckoff & Sidowski's (201) demonstration of probability discrimination above the level that would correspond to matching behavior. In that study the guessing aspect of the S's behavior was minimized.

The study of Brown, Clarke & Stein (29) which was designed to investigate spatial generalization may have some relevance to the influence of set on probability learning. Ss were "tested in a simulated horse race-betting situation where seven horses are symbolized by seven lamps spaced at equal intervals along a horizontal line in front of S." The S was asked whether a given horse will win or lose a race against other imaginary horses not represented by lamps. The middle horse "won" 80 per cent of his races, while the other horses "won" only 20 per cent of theirs. "The

lamps nearest the 80 per cent winner . . . evoked significantly more 'win' responses than more remotely located lamps, even though objective win frequencies for all but the central lamp were identical." Obviously, matching behavior is not as stable as some would like to believe.

Gardner (64) offers a new explanation of matching behavior. It is possible to attribute matching behavior to the S's tendency "to equalize the two proportions of misses. . . . Thus, by this mechanism of error equilibrium the matching behavior found with two choices is seen as a special case, and quite different modes of response are predicted for three or more choices." Gardner predicts that matching behavior breaks down with "response-proportions greater than presented stimulus proportions for the most frequently presented stimulus, and less than presented proportions for the less frequently presented stimuli." His results were consistent with his prediction, although it may be argued that he did not allow a sufficient number of trials.

Theoretical articles.—Restle (149, 150, 151) in a group of three papers has sought to extend a descriptive, mathematical theory to several different empirical areas. He (149) offers a mathematical theory to account for the relation between reinforcement probability and speed of learning in guessing experiments. His theory bears close resemblance to that of Estes in that both formulations describe the stimulus situation as consisting of a set of cues, each of which is capable of being conditioned to one response at a given time. Some of his major points which differ from Estes' formulation are (a) "A cue may be 'adapted' and rendered nonfunctional during learning," (b) "The probability of a response is the proportion of the unadapted cues conditioned to it," (c) "The degree to which a cue is differentially reinforced determines its validity," and (d) "The rates of conditioning and adaptation depend on the validities of the cues available." Restle analyzes the empirical accuracy of his theory by getting a close fit between his theoretical predictions and empirical estimates of cue validity as a reinforcement probability. He is also successful in "predicting" results of published research by using as a reference point the results of only one of several groups. But in a disarmingly frank manner Restle notes that these "results do not constitute evidence that the theory is correct, since the theory was formulated in the light of those results."

In another paper (150) Restle attempts a resolution between the so-called place-vs.-response question. The resolution, which is similar to others (e.g., 100), shows that the place-vs.-response controversy is a pseudo problem. Restle dismisses the controversy as being incorrectly drawn; "There is nothing in the nature of a rat which makes it a 'place' learner, or a 'response' learner." Restle assumes "that maze running depends on a multiplicity of cues, and that the rat learns differential responses to relevant cues in a maze just as he would in a discrimination box." The resolution of this controversy does not stem from any statistical learning theory orientation, but instead from a tough-minded epistemological analysis. Restle concludes in terms of his own theoretical formulations that in the typical place-

vs.-response type learning situation "irrelevant cues are adapted during learning, and the rate of learning depends on the *proportion* of relevant cues."

Finally Restle (151) applies his statistical discrimination learning theory to learning set data (e.g., 77). His theory emphasizes the cue value of rewards and distinguishes between abstract and concrete cues. Again Restle is careful to emphasize the *ad hoc* nature of his equation.

Green (71) proposes a modification of the Estes-Burke theory by hypothesizing a different value for θ (rate of learning in terms of ratio of stimulus elements sampled per trial) under reinforcement and nonreinforcement. This the author suggests explains the common observation of increases in response probability in the presence of negative cues during the early stages of discrimination.

Simon (166) has proposed a model for maze learning behavior. This model is based on the assumption "that learning behavior involves two simple processes: one of discovery and one of fixation." He characterizes a particular maze in terms of two parameters: n , the number of alternative paths at each choice point and L , the number of choice points. He finds his model fits the results of three maze studies.

Purely theoretical articles.—Estes continues to develop the mathematical properties of his model. In one paper (52) his model is extended to cases where π (the probability of reinforcement) is not constant during the course of the experiment and to cases where π may be made contingent upon some responses of the S's other than his last one. Some critics might consider this work with the formal properties of the model to be premature. The model encounters some difficulty in the simple case where π is constant. Also θ has been shown empirically in some cases to depend on π . But it has been the custom for statistical models (and for that matter, all theoretical formulations) to outstrip the data. This can be useful if the new features of the model suggest new empirical problems. But it should not be forgotten that the forward progress of the formal system is occurring at a time when the model is having difficulty digesting some discordant findings (e.g., 3, 64, 200).

Burke & Estes (32) in an article directed at sophisticated mathematicians (of which this writer is definitely not one) trace the course of the conditioning of a single stimulus component. In another article (99) written by mathematicians for mathematicians, it is noted that Estes' model can be treated as a Markov process with a finite number of states. It is also noted that the Bush-Mosteller model (33) is a limiting form of the Estes model.

Concluding remarks.—There is little doubt that statistical learning theory is here to stay. There is much doubt, however, as to the general and specific influences it will exert on the psychology of learning. One problem that still remains to be clarified is how statistical learning theory differs from other theoretical orientations. It is interesting to note that Estes (51), in his review of Spence's *Behavior Theory and Conditioning*, remarks that

Spence brings "... off a quantitative prediction." Genuine quantitative predictions are obviously not the exclusive property of statistical learning theories. Is the difference between the theoretical program of Spence and that of Estes one of kind or one of degree? A clear answer to this question will contribute much to the understanding of the contemporary scene in theoretical psychology.

The future might hold an answer to another interesting question. The kind of empirical phenomena the mathematical learning theorist has dealt with is rather complex. The appeal of such problems as probability learning has not been their psychological simplicity but instead the quantitative simplicity of the data it produces. This strategy may ultimately backfire if the theoretical model that is developed is difficult to extend to other empirical areas for the simple reason that it reflects a complex combination of the effects of basic psychological mechanisms combined with factors that are relatively unique to the complex experimental situation. At present statistical learning theory has dealt with facts from a relatively narrow empirical area. As it extends itself it will be possible to observe the influence of its initial choice of empirical problems.

Are mathematical models enough for psychological theory? Most statistical learning theorists would probably answer this question in the affirmative. Yet one can detect a difference between the approach of some model builders (33) who puritanically deal with a quantitative description of behavior alone and the activities of other statistical learning theorists who inject a conceptual analysis of behavior into their model. At best, the explanatory principles offered by this latter group are modest and limited when compared to other psychological theorists.

It might be argued that if no attempt is made to understand basic psychological processes, aside from their quantitative expressions, then statistical learning theory is predestined to produce at best a large number of detailed descriptions of relatively isolated behavior events. The mathematical fit often appears to be the only concern of some mathematical learning theorists. If the behavior fits the model, then everything is fine. If, however, the behavior does not fit the model, then this fact is usually noted—and ignored. Why does matching behavior occur in some probability learning situations and not in others (e.g., 200)? The answer to this question would seem to demand some theoretical assumptions about the nature of behavior processes that have been up to now subsumed under such topics as motivation, response competition, discrimination learning, etc. Can the problems that have given birth to these concepts be safely ignored by the mathematical learning theorist?

In fairness to the mathematical learning theorist, a theorist must initially wear blinders. If he paid attention to every apparently discordant result he would never get his theory off the ground. The question now is whether the statistical learning theorists have not reached a degree of maturity to come to grips with the problems they may have felt were strategic to bypass, if

not ignore (e.g., the constancy of θ as learning progresses, the failure of some probability learning situations to produce matching behavior, etc.).

The theorizing of Estes and Restle is not typical of all researchers who utilize the mathematical model approach. Although they are much involved in the mathematical aspects of their formal system, they do not turn their backs completely on the conceptual problems that are the concern of non-mathematical theorists. The statistical learning theorist is obviously not committed to treat such problems as motivation, verbal processes, etc. in any traditional manner. The question is whether such problems should be faced up to or ignored. If the writer can read the signs of the time he would place Burke & Estes and Restle into the corner, and a small corner at that, of statistical learning theorists who will maintain contact with and perhaps even come to grips with many of the traditional conceptual problems of psychology.

COGNITIVE THEORY

Cognitive learning theory of the sort Tolman (185) espoused, as well as the Gestalt variety, is either sleeping or dying. This seems to be the only conclusion to draw from the limited amount of activity each has generated over the past year. Why has Tolman's cognitive theory failed to realize the potentialities that seemed to be inherent within that brilliant book *Purposive Behavior in Animals and Men* that was published over a quarter of a century ago? There are several reasons for this. The phenomenological flavor of its intervening variables suggested a level of theoretical explanation which in fact did not exist. Cognitive psychologists were for the most part lulled into theoretical inactivity. The negative attitude towards S-R psychology dominated their activities to such an extent that they ignored the development of their own theory. And finally no cognitive functionalists were available to provide empirical and theoretical nourishment to cognitive theory.

Cognitive psychologists have investigated several aspects of rote learning. Most interesting was the study of Rock (154), who questions the frequency principle in paired-associate learning. The traditional procedure was used for control Ss, while for experimental Ss unlearned pairs were removed and new ones substituted on the next trial. No significant differences in rate of learning were found in two similarly designed experiments. This phenomenon needs further investigation. The experimental methodology was primitive (flash cards and a metronome). But what is more important uncontrolled factors might have been operating. The individual Ss in the experimental group would continuously avoid having to learn items that were difficult. An item that was not learned immediately was eliminated and another item, possibly easier, was substituted. Another possibility is that novelty facilitated learning.

Wishner, Shipley & Hurvich (198) demonstrate that the perceptual differentiation of a list of nonsense syllables will alter the shape of a serial

position curve. Kaswan (94) investigated the influence of perceptual fittingness on paired-associate learning. He finds that fittingness facilitates learning in both intentional and incidental learning. His prediction that fittingness would be more effective in incidental learning was supported by a one-tail significance value at the 10 per cent level. Bahrick (8) reports evidence from an experiment also involving incidental and intentional learning to support the idea that breadth of learning is influenced by drive level.

Muenzinger & Baxter (128) initially trained rats to respond differently to shock (approach it to get food or avoid it) in the presence of a distinctive cue. When the cue was present during discrimination learning the Ss' performances were related to the kinds of responses they had previously learned to shock. Muenzinger *et al.* offer a "meaning" interpretation of their results, which they feel has the virtue of avoiding the use of the "fear" concept.

Pereboom (144) reports a goal gradient on the very first experience a rat has in a 16 ft. runway and a 14-unit T-maze. The author assumes that the rats are fearful but they proceed ahead because of curiosity. Fear is gradually eliminated and the loss of fear generalizes to the later portion of the apparatus, thus reducing the running time. This idea bears similarity to Kanner's (93) explanation of the California latent learning phenomenon. Perhaps the use of tranquilizers with rats would help test the validity of the hypotheses of Pereboom and Kanner.

Petrinovich & Bolles (146) found rats able to perform a temporal alternation in a T-maze when delays between trials were increased to several hours. "Traditional S-R mechanisms do not appear capable of explaining this behavior. Therefore, it seems reasonable to assume that some sort of symbolic process underlies it." This symbolic process must be related to drive conditions, because only animals which lost weight were able to learn the problem.

Mason (118) disproved an S-R interpretation of the IREE which has all been forgotten (40).

Finally, some sort of prize should be given to Goodson, Scarborough & Lewis (69), who wrote a pure cognitive article uncontaminated by anti-S-R hostilities. They found in a simple, neat experiment that expectancy responses extinguish rapidly. Conceivably their technique could be developed to enable a systematic development of this theoretical construct which unfortunately never matured.

The above does not include all the contributions of the cognitive psychologists. Adding to this list other articles (e.g., 153) that have been previously mentioned, which might be considered cognitive in spirit if not in name, would still not produce a healthy number of contributions. Some cognitive sympathizers might offer as an excuse that the neobehaviorists have adopted parts of cognitive theory by their emphasis on the r_g - s_g mechanism. But that is sheer rationalization and an expression of the illness that has befallen cognitive theory.

INTERTHEORETICAL DIFFERENCES

The plan to treat the experimental results of the past year in terms of their implications for individual orientations should not hide the fact that different orientations sometimes produce competing interpretations of certain phenomena. Two of these intertheoretical differences will now be considered.

Secondary (conditioned) reinforcement.—One problem that cuts across "school" lines is whether a stimulus, in order to function as a secondary reinforcer, must first be established as a discriminative stimulus. According to Keller & Schoenfeld (98) it must. Hull (89) had postulated that a neutral stimulus could become a secondary reinforcing agent if it occurred "... repeatedly and consistently in close conjunction with a reinforcing state of affairs."

McGuigan & Crockett (119) sought to test the relative merits of these apparently contradictory formulations. They trained two groups of rats in a discrimination apparatus; Group I had a choice between a black and white end box, only one of which contained food, while for Group II both end boxes were of the same brightness and both contained reward. That is, a stimulus was established as a discriminative stimulus for Group I but not for Group II. A Y-maze which had a black and white end box was used during the test series. The results suggested that the stimulus (white or black end box) that was present with the reward became a secondary reinforcer for Group I but not for Group II. The results were interpreted to support the Keller-Schoenfeld hypotheses. There are possibly two defects in this experimental design. Firstly, the discrimination group (Group I) may have developed avoidance tendencies toward the negative cue during initial training which would lead to the selection of the positive cue during the test. Secondly, since Group II (nondiscrimination group) experienced the negative cue for the first time during the test they might have been reinforced by going to it on the basis of "perceptual curiosity" (11).

Wike & McNamara (195) report (a) that a secondary reinforcing stimulus is conditioned maximally when it functions as a discriminative stimulus and (b) a weak secondary reinforcing stimulus may be established without discrimination training. They also find that the close and consistent presentation of a stimulus with a primary reinforcing agent (food) does not invariably produce a secondary reinforcing stimulus. In general then their research fails to provide unconditional support to either the position of Keller-Schoenfeld or of Hull.

Stein (179) reports findings that are interpreted to be more in line with Hull's view than with the Keller-Schoenfeld hypothesis. By pairing a tone with electrical stimulation of appropriate subcortical regions, Stein was able to establish a neutral stimulus (tone) as a secondary reinforcing agent. His procedure, however, points to the difficulty of relating the controversial question under consideration to a specific experimental procedure. Stein initially trained his rats in a situation containing two levers. During prelimi-

nary training a tone was produced by the pressing of one bar but nothing resulted from pressing the other bar. A slight preference for the no-tone bar was maintained generally through this six day training period. The tone was then paired with the brain stimulation for a total of 400 trials. After this the test series began. The Ss, when confronted with the two levers, demonstrated a marked preference for the tone-bar. Does the fact that the tone was initially nonpreferred mean that tone was a discriminative stimulus? Would the Keller-Schoenfeld hypothesis predict that it would be impossible to duplicate Stein's results if only one bar was used? It is the impression of this writer that these questions cannot be answered unequivocally. If this is so, then purely linguistic problems are involved in what some think to be solely an empirical problem. Unless the theoretical issues are more tightly drawn, the controversy surrounding the relationship between discrimination and secondary reinforcement holds promise to create as much confusion as latent learning once did.

Zimmerman (204), in a relatively noncontroversial article, suggests how it is possible to establish a durable secondary reinforcing procedure. During the first stage a stimulus, such as a buzzer, is followed intermittently by reinforcement with the ratio of reinforcement gradually increasing. Then in the second stage the buzzer alone, with no further reinforcements, serves as a durable secondary reinforcement by being presented intermittently after an instrumental response. The data offered by Zimmerman in support of his method are scanty. Additional data will be helpful not only in evaluating Zimmerman's analysis but also in refining it. Kelleher (96), who also has demonstrated durable conditioned reinforcers, in a personal communication points out that Zimmerman's statement referring to the necessity for changing gradually during the first stage to an intermittent reinforcement schedule is true for only ratio schedules. "With interval schedules, one can shift directly from continuous reinforcement to fixed intervals of 10 or 15 min."

Transposition.—The extension of Spence's discrimination theory (172) to the transposition phenomenon (173) still generates interest and disagreement. Riley (153) believes along with many others that Spence's assumption that an animal in a discrimination problem responds to the absolute properties of the stimulus "is untenable." Riley's viewpoint, however, is not simply negativistic. He argues that the "effective stimulus" in discrimination learning must be described as a relationship between parts of a stimulus complex. This follows from human studies (190) in which the phenomenal brightness of an object is a function of the surrounding illumination level. Riley reports an experimental test of his contrast hypothesis using a modification of Kendler's (103) brightness-discrimination procedure in a modified Lashley jumping apparatus in which both the brightness of the "figure" and "ground" stimuli could be varied. When a far test for transposition was made with the brightness of the ground being held constant, Spence's pre-

dicted reduction in transposition responses occurred. Transposition did not, however, break down in a far test when both the brightness of the figure and ground was varied, so that the ratio between them was kept constant with the original training pair.

The importance of Riley's interpretation stems largely from the problems to which it points. Spence's theory, when it was proposed over twenty years ago, was at best a rough model that suggested the outlines of a theory of transposition. It was far from being a finished theory. It was capable of yielding some crude predictions in situations in which problems of stimulus reception were negligible (i.e., where S would perceive only two homogeneous stimuli). Riley's data show that if the theory is to be generalized to more complex situations, it must consider contextual factors in stimulus perception. But the problem of extension is not the immediate problem facing Spence's theory. Spence's theory must first be applied more directly to the simple situations for which it was designed. To accomplish this, quantitative curves of generalization must be specified for each stimulus in a discrimination problem. Then the explanatory powers of Spence's theory can be tested more adequately than they have been up to now. In order to accomplish this a curve of stimulus generalization (both excitatory and inhibitory) for a specific situation, and perhaps for a specific animal, will be needed.

Riley's interpretation is not necessarily opposed to Spence's formulation. It actually may be a special case of Spence's theory. Koch (104) has made an important distinction that has failed to catch on. He distinguishes between a systematic (theoretical) independent variable and an experimental independent variable. The term stimulus functions in both ways in Spence's formulation. The theoretical stimulus can be co-ordinated to a physical property of a simple stimulus element (as Spence originally had intended) or to a ratio between figure and ground stimulus elements (as Riley proposes). In either case curves of generalization will be needed to generate predictions about the course of transposition. It should be realized that the two problems Riley used do not exhaust all possible tests of transposition. The figure stimulus can remain constant with the ground stimulus varying, or the figure and ground stimuli can each vary along with the ratio between them. Ultimately an adequate theory of transposition will have to consider these problems also.

Rudel (156) also investigated Spence's theory with children in an intermediate size problem. Her Ss were approximately five to six years of age, making this test of Spence's theory inappropriate. One can also question her selection of response measures. But her conclusions are interesting even if they are not closely related to her results. She opposes the traditional dichotomy between "absolute" and "relational" responses in transposition, believing that both are expressions of a more basic discrimination process. Now the problem is to formulate and test such a theory.

ODDS AND ENDS

Some recent contributions fail to fit into our preconceived categories. A report of some recent conditioning work in the U.S.S.R. is contained in a book of papers (165). Otis, Cerf & Thomas (139) and Garcia & Kimeldorf (63) report on two conditioning techniques, the first with goldfish (139), the other (63) with rats using radiation exposure as the UCS with the duration of the stimuli being measured in hours. Thompson (184) reports that isopods (invertebrates) do not show the characteristic improvement in reversal learning that has been demonstrated for such vertebrate forms as turtles. The rapidity of reversal learning may be one of the best behavioral correlates of evolutionary change. Baba (6) reports a relatively rapid method of establishing a discrimination between hunger and thirst drives by shifting the strength of these drives in relatively brief time spans. In his experiment the drive was shifted from hunger to thirst in two hours by having the Ss eat dry food. His Ss were shifted from thirst to hunger in half an hour by allowing the Ss water. Cohen, Brown & Brown (36) established a fear response by using hypothalamic stimulation as the UCS. Beck & Doty (9) report data in support of the idea that a conditioned leg flexion response can occur in the complete absence of the movement of the limb. Henle (81) attacks the proponents of "theoretical togetherness" by stating that eclecticism frequently resolves controversy by ignoring basic issues. Rozeboom (155) returns to the problem of what is learned, which was previously dismissed (100) as a theoretical blind alley. Rozeboom insists that the question is a "straightforward query about the brute facts of behavior" and that the question can be answered by noting changes in the responses to both the CS and UCS during conditioning.

CONCLUDING REMARKS

Learning psychologists are still groping with methodological (both in the philosophy of science as well as experimental sense) problems that they hope will lead to a major breakthrough in understanding behavior. Some believe a breakthrough is now occurring. But nobody is willing to recognize anybody's breakthrough but his own.

However, the gropings of the past year do suggest a pattern. The major features of this pattern will now be given in a modest pontifical fashion.

(a) The range of empirical phenomena that learning theorists are now trying to handle is becoming wider and wider with motivation and perception (at least orienting acts) playing a larger and larger role. Truly learning theories are behavior theories.

(b) S-R psychology dominates the psychology of learning. The success of S-R language is partly due to the fact that it forces its user to think in terms of manipulable experimental variables and observable responses.

(c) Yesterday's physiological models of the learning process have failed to pay off in generating research. Physiology, however, is not becoming less important, but rather more important to the psychology of learning. The

great advances that are now taking place in experimental physiology are, and will continue, establishing a true rapprochement between learning theory and physiology.

(d) Learning theories are paying more and more attention to the S's response. The dream that behavior could be simply related to such independent variables as delay of reinforcement or amount of reward has been shattered. It has been demonstrated (175) that it is not the delay which is the important variable but what the S does during the delay. The consummatory response is probably more important than the amount consumed (194). Guthrie should be given credit for emphasizing this point many years ago.

(e) Skinnerian experimental techniques are becoming more widely adopted. The rate of response measure seems to be at times (124) a more sensitive measure than traditional measures of behavior.

(f) Complicated experimental designs involving complex statistical procedures seem to be offered at times in lieu of theoretical notions.

(g) In general, learning theorists understand each other much better than did their ancestors of two decades ago. Neobehaviorists, S-R functionalists, and statistical learning theorists can communicate easily with each other. Skinnerians also find it easy to communicate among themselves.

(h) Progress is being made!

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INDIVIDUAL DIFFERENCES^{1,2}

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Two papers attack the important problem of developing a conceptual framework for mental abilities. Of the two, Guilford's (63) must be judged much superior to Guttman's (66) on the grounds of "sensible" internal organization and of fruitfulness in suggesting critical studies. Both papers attempt to account for actual correlation data. Guilford classifies those factors that he regards as well-established in five tables that describe "function" or "process": cognition (discovery), production (convergent thinking), production (divergent thinking), evaluation, and memory. An ordering system within each table identifies type of content (figural, structural, and conceptual) and what may be roughly described as "product," i.e., "type of thing known or discovered" or "thing or aspect remembered." Guilford's terms suggest that, at least for the kinds of tasks in which he is here interested, three aspects of a task must be considered in any attempt to identify the response. This is an extremely useful reminder that simple systems of task analysis—like "behavior" and "content"—may not be adequate.

One of the characteristics of Guilford's present scheme is that it postulates more abilities than have been identified. The "blanks" in his tables represent fairly well-defined hunches to be checked. However, he has cautioned that the logical necessity of an ability does not automatically make it testable and has pointed out some abilities which for realistic reasons show little promise of ever being tested or isolated from related abilities. Sarason & Gladwin (108) have also cautioned that the system may not prove applicable to other cultures; they view the mentally retarded as coming from or constituting a subculture which may not fit this system. The fertility of the system in yielding hypotheses about real-life functions is demonstrated, however, in Guilford's mapping out of a broad study of the area of creative abilities in the arts (62) with hypotheses deduced from the general structure of intellect. More recently Guilford has developed a similar structure for psychomotor abilities (64), this time complete in one matrix. The columns represent types of ability and the rows parts of the body. Again there are empty cells which may have meaning. This system should also prove useful though it is considerably less audacious in its formulation.

In contrast, Guttman's scheme seems to lack a kind of internal consistency and—more critical—to fail to reveal the hunches that need to be

¹ This review attempts to cover the period May, 1956, to May, 1958, except for those papers reviewed in the chapter on Personality in the *Annual Review of Psychology*, Vol. 9 (1958).

² The following abbreviation will be used in this chapter: MMPI (Minnesota Multiphasic Personality Inventory).

checked in order to test or correct the scheme. Possibly this is saying merely that it is no "scheme" at all. He has culled from available correlation data a number of approximate simplexes (sets of tests assumed to be similar in "kind" but different in "complexity") and approximate circumplexes (sets of tests assumed to be similar in complexity but different in kind) whose intercorrelations meet his criteria. He classifies these as verbal, numerical, and visual abilities, and under each heading occasionally suggests his notion of what is common (or different) in the set. Let us illustrate the problem of internal consistency with some of the approximate simplexes he identifies. (Incidentally, sloppy editorial work is indicated by test names that differ from those in the source, omission of test code numbers, and incorrect test code numbers.) Using data from Thurstone & Thurstone he identifies the tests Same or Opposite, Vocabulary, Completion, Association, and Synonyms as making up the simplex "understanding single words." He then identifies as "understanding combination [*sic*] of words" the simplex made up of Same or Opposite, Vocabulary, Sentences, and Paragraphs; lists Same or Opposite as a member of the simplex, "increasing abstractness of verbalization"; and finds Vocabulary, Association, and Synonyms members of still another simplex, "abstractness of verbalization (in another direction)." His statement (p. 395) "That it has been possible to conceptualize (and find) simplexes for different kinds of verbal abilities—as indicated by the captions we have chosen. . ." suggests that he regards the Thurstone tests of Vocabulary, Association, and Synonyms as markers for at least two "different" abilities when they are studied in the context of different tests.

Guttman's many papers describing the radex model, which includes as facets the simplex and the circumplex, have established the reasonable expectation that a model that is worth such detailed and sophisticated explication should, when used as a tool, give us at least a few new insights into individual differences. If the current paper (66) is an adequate test of the utility of this model, then we believe the model has been found wanting. Within the limits of the approximations tolerated, the various simplexes he lists are "facts." However, to derive from these "facts," categories of verbal, numerical, and visual abilities—with, within categories, the kinds of interpretation problems we have suggested—does not advance the study of individual differences. Guttman describes (p. 392) this search operation: "Whenever a battery of tests was described by its author, the tests were first classified as well as possible according to the facet of kind of complexity [*sic*]. If five or more tests could be found of the same kind, their intercorrelations were examined to see if they conformed to the simplex pattern." Guttman's list presents the results of this search. The failures (if any) are not listed, and one suspects they might be more enlightening than the successes. Second, this search pattern does not systematically investigate the extent to which the simplex model, i.e., his inverse criterion, fits variables that are, on an a priori basis, equally obviously different in kind. A third criticism is that in this article, at least, Guttman is not able to

account for all the variables in any given study; apparently some of them cannot be described in terms of kind or complexity. What then is missing?

ABILITIES

The interest in high-level mental abilities seems to be coming from two sources. One is the need for selection and training of personnel capable of high-level rather than average achievement. Michael (90) has surveyed the statistical and theoretical models applicable to testing to predict such achievement. The other is the familiar belief that intellectual functioning can be better understood by examining it in people where it is best developed (63). Cutter (34) pushes this notion quite a bit farther, proposing that we view intellectual potential as equal for the whole species and pegged at about the level of Einstein, and that we regard all deviations from this level as due to varying degrees of impairment, hereditary or otherwise.

Both these interests logically lead to increased attention to creative abilities. Guilford (62) has outlined a sizable set of testable hypotheses about the nature of creative abilities, deduced from his structure of intellect. Berger, Guilford & Christensen (14) tested hypotheses about planning ability. Seventeen factors were extracted, of which four were new and believed unique to planning: ordering, elaboration, perceptual foresight, and conceptual foresight. Owen, Schumacher & Clark (102) used criterion groups of creative and noncreative machine designers to develop a battery of predictor tests. They were unable to obtain very high validity, but they undoubtedly produced more worthwhile information than if they had selected extreme criterion groups so that an apparently highly valid predictor could have been produced. Springbett (113) has developed a memory test, based on Hebb's view of consciousness, that is supposed to measure creative thinking. It is an ingenious little test but its association with creativity appears at present to be a matter of faith.

The personalities of creative people continue to be a matter of interest. Barron (6) whose earlier work on originality among Air Force officers had been criticized for failing to take into account intelligence (*Annual Review of Psychology*, Vol. 8, 1957), partialled out the effects of intelligence and found 23 personality variables still related to scores on his originality tests. He also examined subjects with wide discrepancies between originality and intelligence, the most general finding being that people whose originality outran their intelligence tended to lack ego control. Drevdahl (40) studied students rated for creativity by their teachers. He obtained support for some of the ability and personality factors Guilford had associated with creativity, but not many. Drevdahl & Cattell (41) factor analyzed the 16 PF tests of 153 eminent artists and writers. They differed from the norm on 11 factors in much the same fashion as the creative scientists that had been studied earlier. Generally they showed withdrawing, asocial traits, combined with high ego strength.

The study of perceptual and psychomotor abilities is also going beyond

a concern for simple tasks performed by naive subjects. Fleishman (50) has summarized his work on developing psychomotor selection tests for the Air Force, listing the unitary abilities found. In his factor analytic comparisons of psychomotor performance at beginning and advanced levels (48, 49) he has found the same ability factors to be present at both levels of performance, but with different loadings. Adams (1) achieved essentially similar results using a multiple regression approach. He reasoned that the operations don't change during the course of skill acquisition, but that the early tests mainly differentiate between people with different amounts of prior incidental learning. Fleishman (50) has suggested, however, that there still may be abilities which appear only at highly skilled levels and has work in progress using predictors, such as a test of integrating complex learnings. In the perceptual area Michael *et al.* (91), working with fliers, isolated three distinct spatial factors.

Measures of intelligence.—Among attempts to measure intelligence that minimize some of the factors dominant in ordinary mental tests is Honkavaara's (70) object-grouping test. By categorizing types of groupings as abstract, concrete, and dynamic-affective, he was able to obtain some clear differences between bright, normal, and backward children, but his claim that this device measures intelligence in terms of organic development and that it is learning-free certainly remains to be demonstrated. The "ideal" conception of a test situation in which all the subject has to do is sit there and be bright or stupid was approached by Blank & Rawn (17) with a test in which the subject merely points at pieces of human figures that belong together; its reliability proved ridiculously low. McColskey (86) tried to develop a test that would go beyond simple pass-fail items and present problems which could be scored according to differences in approach and apparent cognitive functions, but the test is something of a jumble and has no check on reliability. Perhaps the most promising new way of studying intelligence is one primarily designed for studying individual differences in problem solving. John's (73) problem-solving and information apparatus (PSI) is a self-contained electronic game yielding eight objective measures of work output, five measures of efficiency in obtaining and utilizing information, and six measures of kind of approach. Studies on sizable samples so far indicate the measures are reliable and have some psychological meaning.

Studies of specific factors.—French (52) factor analyzed 13 supposed pure factor tests and got back 12 of the same factors. Only "space" failed to come through. Two mathematically derived fluency scores were studied by Rogers (104) and Hofstaetter, O'Connor & Suziedelis (69). Both studies found the scores to measure verbal intelligence, but the latter study found the scores to have little generality over different classes of words. Guilford, Kettner & Christensen (65, 78) carried out an elaborate factor analytic study of hypotheses about the nature of the general reasoning factor and arrived at the conclusion that it means the ability to structure problems. Of interest in relation to his study is Lawrence's (82) analysis

by use of an interview technique of the errors made by 84 grade school children on an intelligence test. He found the most common source of error, accounting for 30 per cent of the errors, to be inadequate formulation of the task, which sounds like the definition of general reasoning given above. Keehn (76) made a factor analytic test of the hypothesis that Thurstone's flexibility of closure could be more simply defined as the ability to hold an unfamiliar figure in mind and obtained very clear-cut confirmation. An extremely thorough review of the place of vocabulary in mental measurement is given by Yates (125).

Group differences in intelligence.—In their review of research with the Wechsler-Bellevue during the period of 1950 to 1955, Guertin, Frank & Rabin (61) called the establishment of sex differences the major finding and saw social class differences as the most promising area for further research. These plus the problem of intelligence in old people have continued to be the most vigorously studied questions during the period of this review. Sarason & Gladwin (108) have reviewed research on sex, age, and cultural differences with reference to mental subnormality. Miner (94) also reports on such differences from a country-wide sampling survey in the United States using a vocabulary test.

Brown & Bryan (20) reviewed studies specifically dealing with sex differences in intelligence and suggested that if these differences are treated as qualitative this would permit intelligence tests to be more sensitive to abnormal performance. Two studies, however, have cast doubt on the generality of such sex differences. Morgan (96) found no significant differences on a test of problem-solving; Miner (94) found no sex differences in his vocabulary survey. But Dunsdon & Roberts (43), using four vocabulary tests with 2000 school children, got marked and highly significant sex differences favoring boys. Milton (93) reports a study of the relationship of sex-role identification to problem-solving ability. He obtained significant sex differences favoring males, but found that these tended to disappear when correction was made for scores on several masculinity-femininity (M-F) scales. It is not clear, however, what sort of correction he made. If he merely partialled out the effect of M-F scale performance on the relationship between sex and problem-solving ability he was also removing sex as a variable to the extent that the M-F scale is a valid measure of it, and it would not be surprising that the relationship tended to disappear. However, his separate regression equations for predicting problem-solving ability for each sex had significant weights for the Terman-Miles M-F scale. Possibly this represents an attitude variable like that found by Carey (22).

The difficulties involved in trying to make sense of observed cultural group differences in intelligence test performance were brought under close scrutiny by Anastasi (5) with respect to the particular problem of the relationship between intelligence and family size. She regards the negative relationship as fairly well-established empirically, but finds the hypotheses one might base on it virtually untestable. Two studies have added

information on the nature of cultural differences in intelligence. Woods & Toal (122) examined subtest disparity of Negro and white groups on the revised Beta. Groups were matched for IQ at different levels and significant interaction was found between subtests and races, and between subtests, races, and levels. Mitchell (95) did a factor analysis of cognitive functions for low and high status groups. His principal finding was that the factorial structure for the lower-class group was less clear-cut, more dominated by a general factor. Levinson (83) studied Jewish children, inferring from sociological data (not specified) that they should have an average intelligence higher than that indicated by their Stanford-Binet IQ's. The sample to which he administered the Davis-Eells Test, however, scored lower on it than on the Stanford-Binet. McArthur (85) found evidence that intelligence tests may be biased against upper-class children as well as lower. This hypothesis is that upper-class values de-emphasize speed and exactitude.

Anastasi (4) has written a note pointing up the principal methodological difficulty in studying age changes in adult test performance. Unrestricted sampling within age groups gives an advantage to younger groups because they tend to be better educated. On the other hand, age groups equated for educational level will favor the older group because it will represent a more select group. There being no valid way of adjusting for these two factors, the only solution is to use longitudinal studies. One longitudinal study, but a methodologically weak one, was reported during this period. Nisbet (98) retested 141 teacher training school graduates 24 years after they had been tested at school. The Ss, however, were volunteers and the tests were mailed out. The observed over-all improvement led Nisbet politely to suggest some cheating.

Cohen (29) factor analyzed the Wechsler Adult Intelligence Scale for four age groups and found only the memory factor becoming more important among the over-sixty group. Strother, Schaie & Horst (114) did a cross-sectional study of mental abilities on groups of high ability subjects, all over 70, ranging up to age 88, and found evidence of decline in most abilities. Here it would seem that educational factors would not be significant. Miner (94) ran into something of a muddle with age and education differences in his study. In the over-all sample, older subjects did a little poorer than younger, but when they were divided into two groups according to educational level, the difference disappeared in one group and was reversed in the other. Ghiselli (58) found no evidence of age decline. His samples totalling over 1400 are deceptive, however, for only 29 subjects were 50 or over; considering the modest validity and reliability of his home-made instruments, definite conclusions do not seem justified. Scores on three tests of practical information developed by Demming & Pressey (37) increased with advancing years. The authors suggest that such tests, although not valuable for comparing the intelligence of young and old people, may be valuable for assessing differences in alertness and orientation among older people.

Nature and nurture.—Burt has used the multidimensional analysis of variance approach over a period of years to obtain quantitative evidence on the inheritance of general intelligence. A recent paper (21) gives a comprehensive review of his reasoning, methods, and findings. An interesting point is that in Burt's studies we are presented with dual sets of figures, one based on "one-shot" intelligence tests, the other on "adjusted" IQ's. Subjects whose scores were questioned by their teachers were retested. Consistently the "adjusted" scores attribute more variance to heredity. One wonders if the quality for which the scores are adjusted may not actually be the effects of environment. In their monograph on subnormality, Sarason & Gladwin (108) present a nontechnical discussion of the heredity-environment problem, relating the research to the problem of mental subnormality and showing a constant vigilance for possible cultural determinants of differences. Royce (105) proposes a model based on genotypes and phenotypes, treated, apparently, as projections of the same unities onto different realms of observables. What he is saying seems to be already assumed in Burt's work or Adcock's formulation of the temperament-personality distinction.

RESPONSE SETS

Efforts are being made to "explain" such popular traits as authoritarianism, interpersonal perception, anxiety, and rigidity either largely or in small part in terms of tendencies to respond in certain ways to certain kinds of test items. As reported in earlier volumes of the *Annual Review of Psychology*, a number of studies using slightly different approaches have found evidence that acquiescence, i.e., the tendency to answer positively, accounts for much of the variance of the F scale. More recently, Gage, Leavitt & Stone (56), using the Minnesota Teacher Attitude Inventory as their authoritarianism measure, validated the scores of teachers against pupil ratings and found that items for which the positive response was scored as authoritarian had greater validity than the test as a whole.

The most elaborate study so far reported on acquiescence and authoritarianism, however, makes the picture considerably less simple and suggests that what has been called acquiescence is nothing so patent as a tendency to mark everything "yes" or "true." Christie, Hanel & Seidenberg (27), reasoning that logically reversed items are not necessarily "psychologically" reversed, used item analysis to develop a reversed F scale which discriminated maximally between people scoring high and low on the regular F scale. On a wide variety of samples, the correlation with the F scale was always positive, though often low. These authors also devised a measure of acquiescence based on the probabilities of different patterns of response on a scale of five straight and five reversed items. They found, generally, no response set among low F scorers; but among high scorers on the F scale there seemed to be two groups—those with consistent sets of authoritarian beliefs unaffected by response set, and those with confused partly-authoritarian beliefs and a tendency to agree with more liberal ideas as well.

This latter group could well account for most of the high F scorers found in college student samples and hence for the relationship between acquiescence and authoritarianism in such samples.

A crucial assumption in this study, however, seems to be that a reversed item which discriminates between high and low F scorers is necessarily more "psychologically reversed" than one which does not. It may merely be presented in a way which draws attention to its content more sharply, or it may possess quite another characteristic—be more sophisticated sounding—which makes it appealing to the nonauthoritarian and nonappealing to the authoritarian.

Two related possibilities are suggested by Carey, Rogon & Farrell (23) and Gregory (60). Instead of dealing in reversed scales, Carey and her associates put together a list of common aphorisms half of which expressed authoritarian and half democratic, humanitarian sentiments. For most of the groups sampled there was a positive correlation between F scale and reported usage of and agreement with both kinds of aphorisms. If acquiescence is restricted to agreement with familiar aphorisms, then one must question whether the items in a reversed F scale sound as familiar and aphoristic as do their originals—and, on the face of it, the items of Christie's reversed scale certainly do not. In an analysis of a religious beliefs scale which he had correlated with the F scale, Gregory found that items most highly correlated with authoritarianism were those expressing simple, literal, concrete orthodoxy. High F scorers tended to reject philosophical-sounding statements regardless of content—a point which draws attention to the sophisticated sounding statements in Christie's reversed scale.

Although the studies on acquiescence and authoritarianism have shown a good deal of resourcefulness and, generally, more than average regard for adequate sampling, the findings indicate that an article titled "The Psychological Meaning of Acquiescence Set for Authoritarianism" (56) was somewhat premature.

If acquiescence is to be treated as a trait in its own right, it is not necessary or even desirable that its investigation be limited to tests having to do with authoritarianism. Bass (8) used a pseudo-ESP device on which Ss were to guess the responses E was thinking. Responses were yes-no, true-false, etc. He found consistent individual differences in tendency to guess positive responses. However, this internally consistent acquiescence measure was uncorrelated with his earlier scale of social acquiescence, or "Babbity," based on agreement with proverbs (7). Fricke (54) has proposed the K scale of the MMPI as a measure of the opposite pole of acquiescence, the tendency to respond "false." He found that when other MMPI scales were split into subtle and obvious item scales, the differences between scores on the two kinds of scales could be largely accounted for by the K score. From this he inferred that the set to respond "false" should be provisionally recognized as a sign of good adjustment.

Berg (13) found the tendency to give atypical responses generally to be consistent over a variety of different kinds of tests and suggests that it

characterizes an individual's behavior as a whole. Fiske (46, 47) made an intensive study of the tendency to give inconsistent responses, and found that this tendency did show stability over time and over a variety of tests but that it depended on similarity of form of test items and was not related to the content of the items. Although this finding does not eliminate the possibility that intraindividual variability may prove psychologically meaningful, it greatly diminishes the number of tempting hypotheses.

Crow & Hammond (33) contributed to the demolition of interpersonal prediction tests but at the same time added to the stock of possible response-set traits in their study of three response sets involved in interpersonal prediction tests. They found response sets which they called "implicit stereotype," "adherence to stereotype," and "assumed veridicality" (tendency to assume large or small differences between patient's "real" and reported self) to be much more reliable over time than predictive accuracy was over cases.

Some of the fractionating of anxiety into specific varieties also appears to be in the direction of linking it to test response tendencies. Sarason's Test Anxiety Questionnaire, which restricts its range of concern to anxiety over test-taking, is not highly related to measures of general anxiety (87, 107), and shows signs of predicting test performance better than general anxiety measures (107). Dreger & Aiken (39) developed a "number anxiety" measure which is even more obviously a response set—the sum of positive responses to three items. (Incidentally, the analysis of the three items into two correlated factors is probably the most trivial factor analysis of the year.) The total score on these items showed only slight relationship to appropriate aptitude subtests, although they were significantly (negatively) related to college mathematics grades.

In the more remote environs of response-set traits are some which at least at present are less concrete and clearly defined than the personality traits they might be used to redefine. Wiener (121) developed the reasonable-sounding idea of distrustfulness as a trait relevant to performance on such things as picture completion tests where assurance that one is not being tricked would appear necessary for proper motivation. A test of this trait which he put together out of MMPI items did show a relationship with expected Wechsler Adult Intelligence Scale subtests, but an experimental treatment which should have aroused distrust in anyone failed to have any effect.

The work done on acquiescence suggests that the study of response sets can open up new areas of information on individual differences, but that this kind of study is just as involved, tricky, and laborious as most of the others carried out in the field of differential psychology. When one is merely setting out to demolish some "personality" measure by showing that it is mainly a function of some artifact of test construction, he can take a free-swinging approach. The negative case may be relatively easy to establish. But when it comes to explaining why individuals differ in how they respond to this artifact, many new difficulties arise.

PERSONALITY TRAITS

The criterion of internal consistency continues to be applied to scores derived from tests like the MMPI. Comrey (30, 31, 32) and O'Connor, Stefic & Gresock (99) factored items from several of these scales and arrived at the conclusion the cynical psychometrician might expect: these scales are not pure and furthermore the operationally defined scales identified in the analyses often bear only tenuous relations to the given labels. Comrey's studies, if extended, would yield a "new" set of scores for the MMPI item-pool; his present results suggest four (the first two of which are most adequately provided for in the present item-pool): health concern, neuroticism, shyness, and cynicism.

Correlation coefficients have also been used as criteria of the assumed functional correspondence of two or more scores. Nelson & Shea (97) ran the correlations between MMPI and STDCR scores on 52 subjects, obtaining relatively few significant correlations. Comparisons of different measures of a supposed single trait will be mentioned below in discussions of these traits, but negative results abound. Graine (59) compared the Rozenzweig Picture-Frustration measure of conformity with the Edwards Personal Preference Schedule measure of autonomy, reasoning that they should be more or less opposites, but he obtained slight positive correlations.

Both Cattell and Adcock have proposed massive assaults on the genotype-phenotype or nature-nurture problem in personality traits. Adcock (2), assuming that there must be something amiss when one behaves in a way inconsistent with one's innate temperament, feels that it would be of value for developmental and clinical psychology to separate temperamental (innate, physiologically mediated) differences from other personality differences, and he proposes 10 different ways of going about this. His own factorial research has led him to identify as temperament differences: drive and vigor, emotional liability, aesthenia, and kindliness. Cattell, Stice & Krisly (25) propose an elaborate multidimensional analysis of variance design with subjects split according to different degrees of genetic closeness and environmental similarity. Their article includes a report on a trial run made without all the cells of the design filled in. Problems of assessing "genetic closeness" remain, and no matter how detailed the record of environmental history there always remains the possibility that unexamined factors account for the findings.

De Gaudemar (36) has published an extensive historical summary of the development of European type theories; current European work on typology has been briefly summarized by Ostlund (101). It appears from both these reviews that linking physical types with personality types is the main current interest. In this country Humphreys (72) has turned his attention to type theories, arguing that types such as those defined by Sheldon offer no advantages over statistically sounder representations like the discriminant function, which have the advantage of being normative.

Anxiety.—In the 1957 *Annual Review of Psychology*, Jenkins & Lykken reviewed the not-very-favorable findings with respect to the validity of the

Taylor Manifest Anxiety Scale (MAS) as a measure of clinical anxiety. A number of papers, reviewed elsewhere in the present volume, wrestle with the currently "hot" question of whether the MAS does fit Hullian theory, and present the conflicting evidence of the extent to which predictions based on MAS as a drive measure are borne out in various types of learning.

However, the question of the unity and consistency of anxiety as a trait is at least to some extent independent of the terms in which it is defined. Voas' (118) finding that subjects taking the MAS and the same scale within the MMPI showed significant reduction in scores, regardless of which test they took first, is a disturbing piece of evidence. Martin's (88) analysis of 10 different types of anxiety measures represented in 28 variables yielded a weak factor, among eight factors in all, that could be clearly identified as anxiety. The MAS had only small loadings on this factor, and identified instead a factor specific to the test. In contrast, Bendig (11), using an earlier analysis of the items of the MAS into five factors, followed Eysenck in identifying two as neuroticism factors and a third as a combination of neuroticism and introversion. We thus have evidence that the MAS is not a very stable measure under certain conditions, evidence that it does not have much in common with certain other measures of anxiety, and evidence that it is derived from items that may be said to define certain other traits. There is further contradictory evidence as to whether these other traits are neuroticism and introversion. Two studies (10, 51) report high correlations between the MAS and neuroticism as measured by Maudsley tests. Franks (51) also found it related, but to a lesser extent, to introversion. He also follows Eysenck in concluding that anxiety is neurotic introversion and that a good measure of it should be correlated about equally with both traits, Cattell (24), however, brought together results from seven different factor analyses to show that neuroticism and anxiety are distinct factors in that they differ in content and appear in the same studies. He also concluded that anxiety appeared as a single factor in behavioral ratings but in questionnaire and life-record variables as several factors that are organized as a second-order anxiety factor.

Both the Heineman forced-choice version of the MAS and the MAS itself were shown, during this period, to distinguish clinically defined groups (110, 119). Silverman (111) showed that the forced-choice version correlated positively with resting skin conductance and negatively with change under shock, in keeping with the interpretation of skin conductance as a measure of arousal, whereas the MAS did not give this pattern of relationships. However, the findings on measures of reactivity, to which we now turn, do not lend much support to the notion that this forced-choice version would be a better measure of neuroticism.

Reactivity.—New physiological measures of reactivity continue to be added. Edwards (44) developed a refined instrument for recording finger tremor waves along three axes. Such waves were found to be more sensitive indicators of differences among normals, manic-depressives, and schizophrenics than were EEGs. King & Funkenstein (79) studied two types

of cardiovascular reaction to stress (epinephrine-like and nor-epinephrine-like) and discovered a somewhat bizarre relationship between type of reaction and religious conservatism.

The central problem, however, continues to be that of making something meaningful and general out of these varied measures. Duffy (42) cites a variety of studies to show how useful the concept "arousal" or "activation" is for explaining what goes on, but this is quite another thing from showing that arousal can be made a measurable and manageable dimension of individual differences. Fisher & Cleveland (45) have addressed themselves more directly to this problem. They find that the evidence points to the patterning of physiological response tendencies, not the intensity of any particular mode of response, as the most promising kind of individual difference. They offer body-image schema as a psychological concept that makes sense of these differences in patterning. Lacey & Lacey (81) offer additional evidence to support the idea that individuals have characteristic patterns of physiological response to all sorts of stressors, even though correlations between stressors for any single physiological variable may be quite low.

The generally disappointing results from attempts to relate specific physiological measures to psychological variables continue to come in. Plutchik's (103) summary of research with skin temperature presents the same history of inconclusive results that may be written for the galvanic skin response (GSR). Two articles report failures to replicate earlier significant findings—Herr & Kobler (68) with the GSR and neuroticism, Yarbrough & McCurdy (124) with basal metabolism and academic performance. Martin (89) found no significant differences in electromyographic readings between groups identified by Eysenck's tests of extraversion and neuroticism. Two studies that did obtain significant results are perhaps most discouraging of all, for they indicate how far one has to go to obtain such results. Venables (117) found GSR significantly related for normal subjects—but not neurotics—to speed of response on a motor task as difficulty decreased; and O'Connor & Venables (100) obtained an r of $-.40$ between IQ and basal conductance among 35 female imbeciles.

Authoritarianism.—Many of the papers on authoritarianism considered the relationship between it and acquiescence, which was discussed above. Christie & Cook (26) published a guide that attempts to pull together all the published literature having to do with authoritarianism through 1956. They conclude that meaningful relationships have been established between the F and E scales and variables in the areas of attitudes, interpersonal perception, and parent-child relationships, but not in areas like rigidity and tolerance of ambiguity. New evidence does not always support these generalizations. For example, Hart (67) found the F scale related to differences in preference for love- and nonlove-oriented disciplinary techniques, but Gallagher (57) found it unrelated to a measure of harshness of attitude toward children. Millon (92) found authoritarians quicker to settle on a norm in an experiment with the autokinetic effect.

Jones (74) developed the Pensacola Z scale as an authoritarian measure minus the political aspect. Factor analysis revealed it to contain four factors—named dependency, rigidity, anxiety, and hostility—on the basis of which subtests were developed to make up the Z survey. It has a forced-choice form which presumably eliminates simple acquiescence. It has also been shown to foil fakers (74, 75), though this is not to say it is unfakable. Rather, subjects taking the test under instructions to fake "good" were found to lower their scores on anxiety and hostility and raise them on dependency and rigidity so that the over-all score remained about the same.

In a new factor analytic study of the Dogmatism (D) scale, Fruchter, Rokeach & Novack (55) confirmed earlier evidence that the D scale is factorially distinct from the F scale, is related to anxiety, and is independent of liberalism-conservatism. The evidence for this latter point, however, which is the D scale's *raison d'être*, has still been satisfactorily demonstrated only for the high end of the scale. Factor analytic evidence continues to rest on a weak five-item test. Two new tests which might have contributed some evidence—one of which did have substantial communality—were included in the latest analysis, but they were completely ignored in interpreting the factors.

Rigidity.—Levitt (84) has reviewed the research on the water-jar *Einstellung* test, concluding that because of technical weaknesses in the test its validity has never actually been put to an adequate test. In a methodological study undertaken to iron out some of these technical faults, Blank (16) succeeded in getting a split-half reliability of .97 (though he acknowledged such an index didn't mean much on this test) and got the sample loss due to failure on "set" problems down to 9 per cent. By varying the number of jars in different problems in the test without changing the available solutions, Benedetti (12) was able virtually to eliminate the *Einstellung* effect, indicating that the set is not induced simply by repetition of the same form of solution. Frick & Guilford (53) questioned whether the problems induced a set at all, for they found that the greater the number of "set" problems solved, the higher the percentage of subjects who made the appropriate shift. Their factor analytic study failed to show that the water-jar test differed in the abilities it tested from other problem-solving tests.

Hörmann (71) gave an extensive review of research and reported original studies to show the lack of generality of rigidity measures. But Dingman (38), factor analyzing 13 tests of fluency and rigidity given to a large sample, obtained separate and distinct fluency and rigidity factors.

Some correlational studies have yielded results encouraging enough to suggest that the correlates might make better rigidity measures. *Einstellung* subjects proved significantly more susceptible to stuttering in a carefully controlled delayed auditory feedback experiment by Beaumont & Foss (9); the results fit in nicely with earlier findings relating natural stuttering and rigidity. For Brengleman (18), rigid as well as extravert Ss did poorly in tracing designs viewed through a prismatic lens. And in a well-designed

study using somewhat too few subjects, Wertheimer & Aronson (120) brought adaptation to aniseikonic lens, kinesthetic figural aftereffect, and Necker cube fluctuations into a factor analysis of six rigidity measures to clarify the two factors that they had obtained. Lens adaptation loaded on one factor, which they called reluctance to verbalize in unstructured situations, and the other two loaded on their factor of perceptual rigidity.

Other traits.—Clark & McClelland (28) did a factor analytic study of anagrams task as a measure of n-Achievement. They obtained three factors, two of which were judged to be need factors, the other (and primary one) an ability factor. There is further evidence that the n-Achievement Thematic Apperception Test is too much a measure of temporary state to be of value as a trait measure by itself. Birney (15) was unable to replicate some earlier findings of Lowell and of French when a teacher administered the test, but did replicate them when a student administered it. He concluded that the teacher must create so much n-Ach imagery that it obliterates individual differences. Krumboltz & Farquhar (80) found very low consistency of n-Ach scores between the beginning and end of a how-to-study course, which could mean either that the test is generally unreliable or that what it measures can be variously affected by a how-to-study course.

Wysocki (123) found that judges' ratings and a questionnaire measure of introversion-extraversion showed considerable agreement with each other but not with two Rorschach measures. Taft & Coventry (115) found extraverts less accurate in perception of the vertical.

Schaefer & Bell (109) factor analyzed questionnaires, asking about attitudes toward child-rearing and the family, given to 100 student nurses. They were encouraged to find that the five factors they obtained agreed with factors obtained from other analyses of attitudes and behaviors in this area, suggesting that a stable set of dimensions was beginning to emerge. Broen (19) extended his study of religious attitudes to a more heterogeneous sample and found that religiosity broke down into two factors: nearness of God and fundamentalism-humanitarianism. Kerlinger (77) made a *Q*-study of educational attitudes of professors and laymen. The fact that the professors put statements in the clusters he had pre-arranged and the laymen didn't was taken to validate his categories. But one could carry *Q*-technique reasoning a step further and say it only shows that Kerlinger thinks more like a professor than he does like a layman.

Using the somatotype notion, Davidson, McInnes & Parnell (35), in a quite thorough study of personality traits of 100 seven-year-olds, obtained the usual Sheldon relationships, with low correlations. Adcock and associates (3), using Sheldon's measures but not his types on different native ethnic groups and comparing the measures with Rorschach scores, got very little beyond nonzero relationships. Smith (112) took a different approach, relating not body type indices, but ratios between pairs of indices to MMPI scales. Correlations were generally significant, though not always in the expected direction. The endomorph-mesomorph ratio produced the weakest correlations, providing additional evidence that three indices may be one too many.

Like the F scale, the J scale (106), a measure of impulse expression, comes with a wealth of theory attached to it, yielding a number of hypotheses which are testable if you believe in the available testing instruments. It has the distinction, moreover, of not being significantly correlated with the F scale, at least according to early results. Impulsivity is certainly a dimension that sounds good, but it remains to be seen whether this scale will succeed in making something testable of it. Twain (116) factor analyzed 16 different tests thought to be related to impulsivity and got six factors, all of which seemed to be relevant, thus fractionating the concept.

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EDUCATIONAL PSYCHOLOGY¹

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No matter how the educational psychologist may define his subject (see below), he will not be surprised to find his most useful materials, not in this chapter, but in the chapters headed Developmental Psychology, Learning, Theory and Technique of Assessment, Individual Differences, Personality, Social Psychology and Group Processes, Problem Solving and Thinking, or Counseling. Even with such exclusions, however, the remaining field is exceedingly large, and the coverage is correspondingly incomplete. Of the 3500 or more educational serials listed by UNESCO (188), only 95 were searched in any systematic fashion. Of some 4000 articles actually seen in these journals, only 450 were summarized, and of these only 200 appear here. Elimination, moreover, was often far from systematic, being governed in part by sheer inaccessibility and by the linguistic limitations of the reviewer, to say nothing of his biases. To take one instance, the important Russian literature [Razran (142)] was completely neglected.

Summaries available.—The best correction for the bias and omissions in this review is to be found, of course, in the excellent summaries published during the year. Such summaries, invaluable in their own right, are available for the following topics: experimental design in education [Stanley (168)]; general statistical methods in education [Michael, Kaiser & Clark (125)]; techniques for studying education by the observation of group behavior [Taylor & Mitzel (176)]; anthropological and cross-cultural techniques in education [Goethals & Whiting (71)]; sample surveys [Sitgreaves & Solomon (163)]; action research [Corman (40)]; characteristics of the gifted child [Strang (171)]; motivation of the gifted [Goldberg (72); Tyler (186)]; the nature of creativity in connection with the gifted [Wilson (194)]; acceleration [Shannon (160)]; delinquency and other sociological factors in connection with education [Beck (15)]; educational psychology in various countries (United States of America and Union of Soviet Socialist Republics not included) [Noll *et al.* (132)]; the effectiveness of the core curriculum [Michelson (126)]; the relative effectiveness of directive and nondirective teaching [McNeil (122)]; the problem-solving approach in teaching [Gross & McDonald (80)]; lecture vs. discussion [Stovall (170)]; science teaching, including methods, scientific interests, and enrollment [Oburn *et al.* (133)]; psychological processes involved in science teaching [Johnson (100)]; psychology of reading [Gray (76)]; and factors associated with reading disability [Johnson (102)].

PSYCHOLOGY IN EDUCATION

Theoretical issues.—A discipline that is difficult to define on other grounds may come to be defined by the journals in the field. The *Journal of*

¹ The Survey of the literature pertaining to this review was completed April, 1958.

Educational Psychology, now coming under the management of the American Psychological Association, will consider most phases of child development (personality, interests, mentality), the school's impact on such development, learning, and methods of research.

Viewed as an effort to contribute to general knowledge, educational psychology is taken to be the study of those social mechanisms, particularly those mechanisms available to institutions, by which the learning mechanisms within the pupil are engaged. As an applied science, our subject is considered to include the description of educational growth, as the dependent variable, and all the factors that affect it, as independent variables.

In the United States, there has been some worry about the role or usefulness of educational psychology. A symposium at the 1957 meeting of the American Psychological Association debated educational psychology's claims to a unique or genuine function. A current broad attempt to make psychology more useful for education may bypass educational psychology to some extent. In a study of the charges made against educationists, Auerbach (4) found unanimous agreement that educational courses, including those in psychology, overlap unconscionably.

Burch (25) held that in liberal education the prime obligation of the teacher is to reproduce the educated man he finds within himself. This he does chiefly by the spontaneous expression of his intellectual interest. This thing that Burch claims teachers ought to do is the very thing that Stephens (169) claims they are naturally led to do through the spontaneous operation of the basic mechanisms of schooling.

Approaches to research.—Reviews of research techniques are listed above. Ryans (150) has claimed that much of the research in our field is concerned with routine practical services. Symonds (173) laments the present anarchic conditions and suggests a co-ordinating council for educational research. Goethals (70) has called attention to the vast amount of interaction that must be studied in a comprehensive investigation of education. In his review of action research, Corman (40) suggests that this approach is chiefly useful for obtaining leads and for stimulating interest. Becker & Geer (16) show some of the advantages of participant observation over the interview, where the observer is at the mercy of the respondent's bias.

ACADEMIC AND INTELLECTUAL GROWTH

Measurement.—As a step toward test construction, some proposed goals of education were examined [French (64)] and, for each proposed goal, illustrative types of behavior are given.

Harris & Rarick (85) described an apparatus for the continuous measurement of pen pressure in handwriting. Pressure was not related to legibility.

Teachers' attitudes to general problems in testing were reported by Torbet (181). Most teachers thought that testing should serve a teaching purpose, and 75 per cent favored the essay test. In some forms, by the way, the essay test is achieving considerable objectivity. Two independent studies, [Pidgeon & Yates (140); Grant & Caplan (75)] reported high agreement

($r = .98$) between judges in the case of factual materials, and moderate agreement ($r = .80$) for comprehension.

In the matter of objective examination, Bakan (8) found little gain from trying to make students test wise, or from permitting them to check several possible answers as true and penalizing them for the number of choices used. Metfessel & Sax (123) reported that several published tests tend to concentrate true choices in one or two choice positions, and Lawrence (109) found that when children guess they tend to choose the first distractor choice.

In comparing different tests, Pringle & Neale (141) found fair agreement ($r = .80$) between the Gates and the Schenkel reading tests, with fewer zero scores for young children on the Gates. Wysocki (198) found that the traditional intelligence tests showed some agreement with total number of responses on the group Rorschach ($r = .40$), and with the percent movement responses ($r = .30$ to $.40$). Coaching can affect intelligence test score, the most effective device being the discussion of errors [Heim & Watts (86)].

The Progressive Matrices Test was tried in two American cities [Sperazzo & Wilkins (167); Tuddenham *et al.* (185)] and results showed regular age progression, reliability from .87 to .94, marked socioeconomic status differences and, in one case (167), marked Negro-white differences. A few older children reached the ceiling.

Basic constituents of school subjects.—French (63) selected 13 tests each of which, on a priori grounds, should measure one of the factors he had previously identified. In a test given to military cadets, 12 of these did define the expected factor. Spatial abilities turned out to be impure. Three new factors appeared: grades, interests in science, and leadership-sociality. In a similar study, Hunt (95) failed to find support for the six reading skills postulated by Davis. One factor would seem to account for all the relations. Mathematical subjects in the grammar schools (England) include a single factor that can be differentiated from intelligence [Wrigley (197)].

Gifted children.—One volume of the *57th Yearbook of the National Society for the Study of Education* was given over to the gifted child. In one chapter, Wilson (194) summarizes the classical material on creativity and adds summaries for many recent studies, some of them as yet unpublished. He also suggests ways of encouraging ideational fluency, originality, and redefinition. The other reviews, listed earlier, add to the material to be found in textbooks on individual differences.

Gifted children who have been accelerated in the early grades are found to hold their own in later grades [Morgan (130)]. Many advantages and only a few disadvantages of acceleration were noted in Shannon's (160) review of 16 studies. From a longitudinal study, grades three to seven, Haggard (81) found some loss of creativity and a deterioration in attitude toward adults, attributing this to the pressure for academic attainment, including pressure from other pupils.

Intellectual and academic disability.—Academic failure is the most frequent reason for referral to child guidance centers [Gilbert (68)]. Special-

class children, aged 6 to 16, were aware of their limited academic ability but used no derogatory terms in describing their class [Johnson & Ferreira (99)]. Schenk-Danzinger (155) sees little chance of reducing the present failure rate of about 30 per cent in the Austrian schools. In Belgium [Hotyat (92)], about 15 per cent of children are retarded by one year at age of eight and 21 per cent are retarded by age 11. According to Schonell (156), the corresponding figures for Australia are 7 per cent and 11 per cent, although the failure rate for children under 85 IQ is over 40 per cent.

Johnson's (102) review of disability in reading included some 179 titles. Poor auditory discrimination seems to play a part, but there is little clear evidence for a retinal or neural basis. Emotional disturbance and difficulty in oral speech are both related to poor reading, but the causal pattern is in no way clear. The February, 1958, issue of the *Journal of Educational Research*, (Vol. 51, No. 6) is devoted to studies of reading and reading disability. Worrisome problems in reading include ignorance of the sound of letters, lip movements, and using the finger as a pointer. Poor readers are marked by poor auditory memory span. Their learning is somewhat improved when tracing of words is permitted. Buswell (28) held that failure to develop perceptual skill is responsible for much reading disability at the college level. Phonemes typical of words difficult to spell were identified by Petty (138), but the errors did not occur in spelling the phonemes themselves.

Predicting academic achievement.—In line with the well-established constancy of academic attainment [Thomae (177); Olson (134)], performance at one educational level continues to be a useful index of subsequent performance [Garside (67)]. Even with intelligence held constant, successful high school pupils surpass the less successful when both go to college [Swenson (172)]. Foreign students who have questionable undergraduate records upon admission to graduate work are more likely to find themselves in trouble later on [Hountras (93)].

Cooper (39) found high correlations ($r = .53$ to $.77$) between intelligence and achievement in the case of bilingual grade four children in Guam. In general, the "culture-free" tests were less predictive than the standard tests in English. Woodcock (196) found that a special artificial language test was superior to a regular intelligence test for selecting candidates (grade four) for a program in remedial reading.

Tests of study habits or skills have been rather poor predictors of subsequent achievement [Michael *et al.* (124); Ahmann & Glock (1); Chahbazi (33)]. Carter (32) has revised the Inventory of Study Habits, however, and now reports a correlation of $.53$ between the mechanics of study and achievement in high school.

Some studies cast doubt on the value of expert judgment. When cold test data were interpreted by experts, the predictions did not improve and may have deteriorated [Garside (67); Truesdell & Bath (184)]. Among the experts, the less experienced were better. High school students were able to predict the academic performance of each other to the extent of $r = .80$ [Keislar (104)], and were the equal of teachers in being able to select other students who would leave school or who would "make" the honor roll

[Ullmann (187)]. Undergraduates duplicated ($r = .88$) the judgments of trained clinical psychologists in judging the significance of statements made by schizophrenics [Hunt, Jones & Hunt (96)]. Grade teachers excelled psychologists in guessing the kinds of behavior that seventh grade children would approve or condemn [Goertzen (69)]. And training in the interpretation of clinical symptoms actually led to reduced accuracy. In discussing the latter phenomenon, Crow (44), following Cronbach (43), suggested that training may have led the rater to spread his judgments over too wide a range.

CHARACTER, INTERESTS, AND PERSONAL ADJUSTMENT

Hemming's analysis (87) treats moral development as a problem of value transmission. This is discussed in relation to the psychoanalytic theories of Freud and Klein. Jacob (98) summarized some 350 studies of values and value change among college students, and Remmers & Radler (143) included much work on values in their summary of the Purdue University youth polls. In both summaries, there was a suggestion of self-centered complacency, and a tremendous urge to "fit in" and to be liked. The high school students approved FBI wire-tapping, had some reservations about free press, and doubted that people should be trusted to decide what is best for them.

Success in teaching attitudes.—According to Jacob's summary, only a few college programs had any effect on values, and these changes seemed to be a part of the whole college climate and not attributable to specific curricula or teachers. Courses deliberately set up to change attitudes were seldom successful. Greenberg, Pierson & Sherman (78) found no reduction on the Ethnocentrism scale following discussions of anti-Negro prejudice. McGuinnies, Lana & Smith (119) reported no difference in attitude toward mental illness following one film presentation. Such a change did occur, however, following a sequence of three films. Cumming & Cumming (45) describe an elaborate campaign to improve community attitude toward mental illness. The program had no positive effect but did bring about marked antipathy toward the reformers. Lodge (112) found no change in "my ideal" described by grade eight pupils following a study of American heroes. Toch & Cantril (180), however, report changes in New Year's resolutions in line with exhortative material read by students. Following a course in child psychology, moreover, students' scores on an attitude scale showed less tendency to dominate or ignore children [Costin (41)].

Interest and popularity.—Carsley (31) showed that more silent reading at home was reported from poorer neighborhoods. Girls liked to read about school and nature, whereas boys preferred humor and how-to-do-it stories. Television viewing occasionally was the stimulus to reading. Frequent television viewers read as much as nonviewers [Mitchell (128)] and, apart from attending movies less, were indistinguishable from nonviewers in a number of traits. Television viewing takes up some 22 hr. per week for the average pupil, more for elementary school pupils [Witty & Gustafson (195)], and is the most frequently reported out-of-school activity [McCullough (118)].

Studies continue to report that sociometric popularity [Wertheimer (191)] is fairly constant from year to year. In high school, persons desired as friends are chosen predominantly from close acquaintances (45 per cent), but 15 per cent were chosen from persons known only by sight [Scandrette (153)]. Young children who are chosen frequently make fewer statements but phrase those statements in more mature patterns. Children frequently chosen are more intelligent [Gallagher (66)] and may be advanced in achievement and degree of pubescence [Davis (47)]. Dull children assigned to a regular class have a poor chance of being chosen as friends but are no more likely to experience positive rejection [Lapp (108)].

PHYSIQUE, INTELLECT, AND ADJUSTMENT: INTERRELATIONS

Passamanick & Knobloch (137) suggest that the lower test scores of Negro children could be explained by the greater frequency of premature births and other complications of pregnancy. Yarbrough & McCurdy (199), in contrast to an earlier suggestion, report no relation between basal metabolism and college grades. Skeletal age, according to Karlin (103), is slightly better ($r = .30$) than reading readiness for predicting first grade reading success. Harris (84) found less marked right-hand dominance in poor readers, especially in grade one. For children in grade one, there is a slight relation ($r = .30$) between endomorphy and IQ. Ectomorphs at this age were meticulous and anxious [Davidson, McGinnes & Parnell (46)]. Holmes & Hyman (90) describe a brain injured man who could not spell from dictation but who could tell when a word was misspelled, thus supporting Holmes' theory of two kinds of spelling ability. Greenberg *et al.* (77) report adjustment scores markedly below normal for pupils in a residential school for the blind, and Laird (107) found elevated Minnesota Multiphase Personality Inventory profiles among the physically handicapped in general.

Anxiety and performance.—Anxiety received much consideration. A few scattered studies suggested something like "number anxiety." Dryer & Aiken (51) found that anxiety about numbers could be differentiated from general anxiety and that such number anxiety was related to poor grades in mathematics. Haggard (81) reported that, among the gifted in elementary school, personal adjustment was better for those doing well in arithmetic than for those doing well in reading. Deficiency in mathematical and abstract reasoning is typical of gifted students achieving below their potential [Barrett (12)], but reading and spelling difficulties are more typical of school retardation in general [Holmes (89)]. In the elementary school, Lynn (115) found that anxiety is especially likely to appear when performance in arithmetic is much worse than performance in reading. At the college level, on the other hand, either type of discrepancy is associated with disturbance in personality [Scarf (154)]. In a military school, personality disturbances are likely to appear when students enter courses dealing with mathematics and highly abstract concepts [Mayo (117)]. Possession of highly stereotyped political attitudes is unrelated to performance in science but is more marked in students doing poorly in humanities than

in those doing well in these subjects [Egner & Obelsky (55)].

In line with Spence's theory, Pickerel (139) found that anxious students surpass the nonanxious in tasks in which there is little chance for error, but fall behind the nonanxious in complex tasks demanding a choice between several alternatives. Exceptional stress in the examination room (rigid, threatening proctoring, difficult practice exercises) will reduce performance, especially for women, and especially for students low in ego-strength and low in achievement motivation [Carrier (30)]. The mere announcement that half the time has elapsed may reduce the performance of slow readers [Cook (37)]. Such stress will lead anxious students to fall even farther behind the nonanxious in comprehension and retention [Chansky (35)]. In general, we can expect better academic achievement from students who have not been judged to be mentally disturbed [Salzinger (151)] and who do well on Gough's Hr scale [Bendig (18)], and who hold values that coincide with those of teachers [Battle (13)]. Students who spend a long time on a test, thereby indicating persistence, do slightly better on the test in spite of their lower IQ's and lower reading ability. Induced persistence does not affect the score [Barch (10)].

THE ROLE OF NONSCHOLASTIC FACTORS

Age.—Age and maturation make a difference. A follow-up of older and younger children who had entered kindergarten at the same time [Baer (7)] showed that by grade 11 the older children had accumulated a better academic record and had better personal ratings from teachers. Elementary school pupils, after three years of French, were still somewhat behind the level attained by college freshmen after one term [Dunkel & Pillet (52)]. Some 43 per cent would have passed the freshman year in French reading, however, and about 80 per cent would have passed in aural comprehension. About 25 per cent would exceed the average of high school students who have had a year or two of French.

Home background.—Heredity had one surprising setback. The handwriting of identical twins is no more similar than that of fraternal twins [Rohracher (146)]. High school leaders tend to come from homes where children are given some responsibility and are consulted on family matters. A broken home seems to have no effect here [Barr (11)]. National Merit Scholars in the United States tend to come from the homes of professional workers. It would take 24 homes of service workers and 250 homes of laborers to equal the "output" of one home from the professional class [Bond (23)]. The children of the college-educated indicate more than the usual number of worries [Drasgow (49)]. The type of secondary school attended (England) may reflect home background [Bene (19)]. Pupils in the Modern Schools (England) show more rigidity than pupils in the technical forms [Miller (127)]. In comparison with pupils from the grammar schools, the pupils in the Modern Schools showed a more positive attitude toward relatives, school, and sports. They were also more satisfied with their scholastic performance, felt less parental pressure, and were less ambitious.

Young children whose absence from home is prolonged by an extensive daily bus trip show poorer school adjustment [Lee (110)]. There is more retardation found in children whose mothers are not at home when the pupils return from school [Renier (144)]. Here, of course, we should not jump to conclusions as to what causes what. There is a correlation between rated excellence of the home and pupil's achievement quotient [Haraoka (83)]. Mothers of such "over-achieving" pupils show greater interest in school, have higher goals for children, believe that children should not interrupt or annoy and that they should be punished for poor marks and disobedience [Ford (62); Drews & Teahan (50)]. The achievement of children remaining in highly disturbed homes is less than that of children removed to foster homes or to an orphanage, but exceeds that to be found in a home for socially maladjusted children [Feinberg & Moscovitch (59)]. Violent temper and disobedience are often found in homes broken by divorce [Russell (149)].

Within one community, Overs & St. Clair (136) found surprisingly little difference in the social class of students in a university, a business institute, and in a vocational high school, but the differences were in the order indicated. Origlia (135) reported many more school failures (51 per cent) from poor neighborhoods in Italy than from the best neighborhoods (10 per cent). In Sweden, also, Blomqvist (21) reported more failures in crowded areas and in larger families. The general condition of the living arrangements was more significant than sheer income.

National comparisons.—A muscular fitness test, failed by only 9 per cent of "European" children, 39 per cent of children in India, 30 to 57 per cent of Canadian children, was failed by 38 to 66 per cent of children in the United States [Kirchner & Glines (105)]. Buswell (29) found that, whereas English children averaged a score of 14 points in arithmetic computation, their California age mates (11 years) scored only four points on the same test. In problem solving, the comparable figures were 15 and 8. Pringle & Neale (141) found English children, aged eight to nine, to exceed the Gates reading test norms for American children by about 12 mo. In contrast to earlier reports, tryouts of the Progressive Matrices Test showed no difference between American and Scottish performance [Sperazzo & Wilkins (167); Tuddenham *et al.* (185)].

METHOD, POLICY, AND SCHOOL ORGANIZATION

Teaching by television.—Tanner (174) points to the "Hawthorne" effect and to the novelty appeal operating on both student and teacher as a confounding influence in television experiments. Siegel & Macomber (161) found little difference in the attainment of college classes taught by television and those taught by instructors in small classes. If anything, the television group was slightly ahead. Similar results are reported by Herminghaus (88) and by Tanner (175) although in the latter case the advantage, if any, was in favor of the groups present in the studio room. Tanner found that achievement dropped slightly when the television situation was unproctored. For the most part, students thought they would have

learned more in the direct situations. After a lapse of three years, material learned by television, like material learned in the ordinary way, was retained remarkably well. In both cases, original scores of 88 or so dropped to 76 [Benschoter & Charles (20)]. Williams, Paul & Ogilvie (193) report that the superior knowledge of a television group persisted through an 8 mo. lapse.

Achievement as a function of facilities.—As has been reported in the past, students doing their work in correspondence courses achieve as much as those in full residence [Dysinger & Bridgman (53)]. These results, consistent from year to year, seem to have great significance for those trying to determine the basic mechanisms by which academic gains are produced.

Bloom & Statler (22) report that within the United States there is a marked correlation ($r = .75$) between per pupil expenditure in a state and the average high school test score of pupils in the state. The latter also correlate highly ($r = .70$) with the educational level of adults in the state. There is no specific claim that expenditures lead to attainment and no mention of the possibility that something like adult interest in education leads both to expenditure and attainment. Anderson, Page & Smith (3) found no relation between size of high school (under 50 to over 600) and over-all high school achievement.

Class size, that ancient issue, ran true to form. In two investigations at the college level [Siegel & Macomber (161); Rohrer (147)] there was no difference in achievement in sections ranging from under 30 to over 300. Students preferred small classes.

Progressive vs. traditional.—Progressive education was very much under scrutiny, and some of its aspects were investigated. Selecting a progressive school in which no formal arithmetic was taught until grade five, Sax & Ottina (152) found an expected deficiency in grades three and four. By grade seven, however, these pupils equalled the matched controls in computation and exceeded them in a special test of arithmetic meaning. Hall & Demarest (82) found that, following the greater use of continuous promotion, there was a reduction of overageness in grade four, but no change in reading ability for the grade. Two years after the abandonment of an individualized marking system and the substitution of a more rigorous system, Baker & Doyle (9) found no difference in achievement. From a survey of 34 titles dealing with the core curriculum, Michelson (126) reported slight and inconsistent differences, the advantage, if any, favoring the core curriculum.

Methods of teaching.—Summarizing some 24 studies of phonics, Smith (165) concluded that 98 per cent of American schools now teach phonics, and that such instruction is generally effective, especially when given in the second grade. Sparks & Fay (166) report a transitory (grades one and two) advantage for pupils taught by phonics from the outset. In grades three and four, special drills in phonics, using phonographs and prepared charts, may add to reading skill [Luser, Stanton & Doyle (114)].

In the teaching of spelling, in grades two to six, Cook (38) found a

slight but inconsistent advantage for the study of lists of individual words as opposed to the study of words in context.

Rudd (148) provides a useful review of the role of homogeneous grouping or streaming, and reports that when a student is assigned to a different group, there is no change in attainment but some deterioration in attitude.

Trueblood (183) found no relation between the achievement of college students and the time spent in earning money. Hountras (94) found that graduate students on probation had been carrying lighter programs than their more successful colleagues. In a general study of University of London students who failed to get a degree, Hopkins, Malleson & Sarnoff (91) suggested extraneous interests and lack of academic drive as some of many related factors.

CLASSROOM LEARNING

General climate or approach.—The conflict between the teacher-centered and the group-centered approach continues unresolved. McNeil's (122) account of some 63 studies is given as separate thumbnail sketches of the data bearing on each approach. With this method, the mutually contradictory claims appear in quite different sections, and no attempt is made to bring out the very real disagreement. Eglash (54) found neither approach to have any effect in reducing opinionatedness. Thompson & Tom (178), on the other hand, found that students in group-centered high school classrooms surpassed other students in knowledge of agriculture, but not in solving problems connected with agriculture. Stovall (170) reviewed 27 studies of lecture vs. discussion, confirming the classical findings of no disadvantage and perhaps a slight advantage for the lecture method in promoting mastery of factual material, but suggesting better retention and perhaps better interpretation from the discussion method.

Motivation and meaning.—Rohracher (146) reported that in improving handwriting an emotional reprimand had more effect than a quiet admonition. Ferster & Sapon (61), using a Skinner reinforcement device, and omitting all grammar and general discussion, were able to teach six adults as much German in 48 hr. of instruction as college students ordinarily learn in one semester. Newman (131) found that students were actually hampered in their learning of electrical symbols when the symbols were presented in groups designed to bring out a common characteristic. Colville (36) found that students were not helped in rolling billiard balls, or in catching a ball on a tennis racquet, when some of the practice time was diverted to the study of the physical principles involved. In contrast to these results, von Wright (189) found that, in learning a pencil and paper maze, sheer trial-and-error was less efficient than pure guidance (being shown the true path, errors being hidden). The best learning was found when the true path was pointed out, with the wrong pathway also indicated. In an experiment that may have some bearing on these conflicting results, Kittell (106) found that grade six pupils profited from a moderate amount of guidance but were not helped, and may have been harmed, by large amounts.

Interference, transfer, and retention.—In classroom applications, Ausubel, Robbins & Blake (5) found that retention of material on Buddhism was facilitated more by a study of material comparing Buddhism and Christianity, than by re-study of the original material. A straight essay on Christianity, although of some help, was less effective than the other two activities. In a separate study, Ausubel, Schpoont & Cukier (6) found that retention of such connected material was not enhanced when students were warned, after the material had been learned, that a second test would be given in two weeks. McKeachie & Solomon (120) reported that 8 mo. after completing a course in psychology, the test scores of students decreased only 7 points from an original score of 78. Benschoter & Charles (20) reported similar retention over a three year period.

In a very practical transfer study, Crofton (42) found that organized experience in working with cardboard helped considerably when it came to working with costlier materials such as wood. In grade six, practice in the discrimination of words led to some improvement in spelling [Mason (116)]. Schwartz (158) reported that practice in the rapid reading of nontechnical material actually doubled the rate of reading technical material.

In the area of emotional control, Torrance (182) found that previous mild exposure to a stress situation (special military rations) reduced the frustration when dealing with the experience in its full intensity. Faw (58) found that classroom stress induced less frustration when preceded by a group-centered approach.

Skelton (164) found better college performance on the part of students who had taken foreign languages in high school. The groups were matched on the quantitative score of the American Council on Education Test. Engle (56), on the other hand, in studying college courses in psychology, found no superiority on the part of students who had had psychology in high school. Success or enjoyment in science courses at the secondary school level was reported to be the chief reason for majoring in science in college [Lovell & White (113)]. Early interest in science was related to scientific interest in the home. At the other end of the educational ladder, Fast (57) showed that kindergarten children excelled nonkindergarten pupils in reading throughout the first grade. In this study, kindergarten attendance was determined by month of birth, and in this way the worrisome complication of parental motivation was controlled. Apparently this factor was not controlled in a study by Allen & Masling (2) showing more frequent sociometric choice of nursery school pupils in grades one and two. This greater popularity, however, was not due to the higher occupational level of parents of nursery school children. Ferguson (60), incidentally, found some parental resistance to a program that restricted kindergarten attendance to every other day.

CHARACTERISTICS OF TEACHERS

Success in teaching.—With teaching success based on observations by principal, supervisor, or an outside investigator, Willard (192) found very little agreement between these observers or between any one of them and

the teacher himself. Observations based on the behavior of students, however, registered enough consistency to permit a correlation of .75 between rated success and a modified Thematic Apperception Test score [Johnson (101)]. Silberman (162) also found over-all consistency between six observers when rating teachers on use of reproof, of praise, and on time spent on various subjects. In this study, the qualities observed had no significant bearing on the pupils' gain in reading score. In an Air Force setting, Borg (24) found no agreement between the ratings given by students and those given by supervisors or by fellow teachers. The latter two ratings correlated .71, however. Teachers (or courses) given a high rating by students showed no consistent tendency to induce students to take further work in the same subject [McKeachie & Solomon (121)].

When desired qualities of teachers are listed in abstract terms and rated for importance, the ratings, according to Robertson (145), contain one important common factor and six subfactors, the latter neatly corresponding with the cognitive, affective, and conative aspects of teaching. Sheer persistence in the profession, as one aspect of success, was reported [Levin, Hilton & Leiderman (111)] to be greater for those who feel rapport with children and who have a strong original "call" to the profession. The intensity of the call, in turn, was related to an early choice of teaching, to family approval of teaching, and to lack of worry about low salary. For elementary teachers, persistence was greater for those from lower socioeconomic status and for those less interested in books or in subject matter. Greater persistence of the less "academic" teachers was also noted for men in secondary schools, but not for women.

Apparently the part played by intellect in success or persistence in teaching is still unsettled. Teachers scoring high on the MAT (Miller Analogies Test) are less likely to have a warm, friendly interest in children (111). In the case of prospective teachers referred to a board as unsuitable for teaching, those scoring high on the MAT are more frequently refused certificates [Seagoe (159)]. High academic grades, however, may go with high grades on practice teaching. Gowan (74) found bookishness of parents, moreover, to be one of the distinctive characteristics of experienced elementary school teachers judged to be outstanding. Other characteristics were exceptional interest in children, superior emotional stability, religious interests, conformity, freedom from blame projection, and lack of resentment against administrators. Goodenough (73) reported a forced-choice technique for rating personality that produced a correlation of .80 with ratings for discipline. There was continued interest in pupil gains as measures of teaching success. Mitzel & Medley (129), working in the elementary school, and Webb & Bowers (190), in aviation training, both found significant differences in gains from class to class.

Teachers' attitudes and interests.—Apart from the measurement of effectiveness, there was some study of devices for observing teachers. In an analysis of preadmission interviews, Burroughs (26) found that the ratings involved a sociability and manner factor, a verbal-intellectual factor, and a

third factor related to interest in children and in teaching. When naive observers (students) are asked to estimate a teacher's attitude toward children, after only a general acquaintance with the teacher, they tend to assign to him attitudes which they themselves hold [Chansky (34)]. The Minnesota Teacher Attitude Inventory was found to have a correlation of .42 with interest in social service on the Kuder scale [Beamer & Ledbetter (14)]. Gage (65) substituted logical scoring keys for the exceedingly empirical published weights and thereby boosted the reliability from .90 to .94, and the correlation with pupil rating of the teacher from .26 to .28.

The interests or concerns of teachers may be analyzed into self-interest, subject-matter interest, service interest, and interest in children (111). The latter trait, already discussed, is more prominent in teachers of younger children, but also differentiates male teachers in training from engineering students [Dille (48)]. In spite of this reported and expected interest in children, Thorpe (179), working with "successful" teachers, and Jackson & Guba (97) working with experienced teachers, both found their subjects to be only average or even below average in "nurturant" needs on the Edwards scale. In both studies, teachers showed strong needs for deference, order, and endurance, and were low in need for autonomy.

How well do teachers understand their pupils? At the college level, Schuhle (157) reported that the typical teacher is apt to underestimate the student's serious desire for self-improvement. Gronlund & Whitney (79) found that teachers could estimate the sociometric status of pupils about as well as they could estimate intelligence ($r = .70$). Teachers good at estimating one were good at estimating the other. In his general review of the area, however, Bush (27) reminds us that detailed knowledge of the individual students has not always been found to be necessary or even helpful for successful teaching. In judging the emotional adjustment of children, male teachers stress maturity and dependability, whereas female teachers stress conformity and modesty. Otherwise there is considerable agreement between the sexes [Beilin & Werner (17)].

EVALUATION

There is a growing and encouraging trend to make classroom tests of basic notions from the psychology of learning and motivation. The classroom application of ideas from social psychology continues at its earlier high level. Some of the boldest, neatest, and most comprehensive studies were done in military settings. Along with much important and rigorous work, there has been a tremendous amount of research on the same tired and trivial issues, much of it indifferently conducted and reported. Negative, paradoxical, and disconcerting results are becoming so numerous and so conspicuous that they can no longer be swept under the rug. Such results demand honest scrutiny, and they invite bold theorizing regarding the basic psychological mechanisms by which the schools function. Interestingly enough, in suggesting that pupils so seldom seem to be harmed, those same results also invite bold experimentation.

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STATISTICAL METHODS

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INTRODUCTION

This review is meant to cover the items through May, 1958, which seem most likely to be of use and interest to psychologists. The year seems to have been characterized by continuing progress on old problems rather than radical achievements, although Tryon's (82) method for calculating communalities may represent a real break-through in factor analysis.

A number of papers of general interest appeared. Because 1957 was the centennial of Karl Pearson, appreciations of this great pioneer in statistics by Haldane (42), Walker (85), and Stouffer (77) provide personal and professional sidelights for those interested in the history of psychology and statistics. Haldane's comments on the controversy between the positivistic Pearson and the materialistic Lenin are timely. Also numerous reactions to the unreconstructed Fisher (32) have appeared, and one by Anscombe (2) is of especial interest. Anscombe shows how Fisher's hierarchy of inferential methods (in ascending order of information: significance tests, the likelihood function, and the fiducial argument) involves a curious oscillation between heeding and ignoring the nature of the sampling process, sequential, fixed, or otherwise, because both the fiducial argument and the significance tests depend upon the sampling rule, whereas the likelihood function does not. Fisher also argues for the asymptotic equivalence of an approximate fiducial argument to the use of Bayes' theorem with likelihood and prior distribution. However, Bayes' theorem does not depend upon the sampling rule. Psychologists are advised to read Fisher with some caution.

ESTIMATION, EXPERIMENTAL DESIGN, AND ANALYSIS

Interval estimation provides an alternative that is often superior to the statistical testing of hypotheses. Bulmer (9) implies this by further suggesting that a distinction be made between confirmation of hypotheses and acceptance of hypotheses. He proposes that a distance function be introduced in the hypothesis space. All admissible hypotheses are then tested and classified as acceptable or unacceptable. If none of the acceptable hypotheses are "near" the null hypothesis, then the null hypothesis is disconfirmed. If, however, all the acceptable hypotheses are "near" the null hypothesis, the latter is confirmed; otherwise the results are inconclusive. It remains to be seen whether or not reasonable distance functions can be obtained, but the approach can frequently be used if confidence intervals are estimated. In this connection Chandler (11) urges psychologists to use more care in distinguishing between confidence and significance, the former

referring to interval estimation and the latter being reserved for the rejection level of statistical tests.

The one-tailed versus two-tailed test argument remains viable in spite of severe bludgeoning. Now comes Kimmel (54) with three alternative criteria for the use of one-tailed tests. His first criterion, namely, that a one-tailed test may be used when a difference in the unpredicted direction would be psychologically meaningless, seems safe enough, but the other two criteria, for all that they may be practically useful, seem to the reviewer to invite confusion and to endanger the integrity of the rejection level. They will also test the integrity of their users.

Estimation.—Ample evidence of the robust character of the analysis of variance F -tests in the presence of nonnormality and heterogeneity of variance has been summarized in earlier reviews. Much less is known about the accuracy of estimates of components of variance when the situation departs from the mathematical model. The indications from a study by Kelleher, Robinson & Comstock (52), which utilized a large body of data on the quantitative inheritance in corn, suggest that the estimates of variance components were generally not greatly affected by nonnormality at the level investigated, and that heterogeneity had only slight effect. The sample of years was, however, too small for a full evaluation of the results, and the precise degrees of nonnormality and heterogeneity were not specified, so that the report is of preliminary value only.

Experimental design.—Murdock (64) has reported an extensive survey and analysis of the problem of the experimental designs and the formulae for measuring transfer of training. Discussing the advantages and disadvantages of the five most commonly used designs, he notes that the standard design, that in which the subjects in the experimental group learn Task 1 and then Task 2 whereas the subjects in the control group engage in a different preliminary activity before learning Task 2, is probably invalid more often than not because of warm-up from Task 1 or general facilitation from learning a similar, though not identical, prior task. Remedial procedures are discussed for this and weaknesses in the other designs. Murdock then goes on to analyze the formulae for percentage of transfer and presents a new one:

$$\text{Percentage of transfer} = \left(\frac{E - C}{E + C} \right) (100), \quad [1]$$

where E is the number of correct responses in the experimental group and C is the number of correct responses in the control group on Task 2. He shows that the formula has an advantage over previously presented formulae in that it covers both positive and negative transfer, with limits of +100 per cent and -100 per cent. Murdock comments that this formula gives very low per cent transfer in most experiments.

Whenever psychologists use two or more procedures applied to the same subject, as in transfer of training, retroactive inhibition, Latin square and

cross-over designs, they run the risk of getting unwanted residual effects from one treatment to another which may invalidate the findings of the experiment. As a rule, neither the logic nor the mathematical model by which the experiment is analyzed includes proper provision for these residual effects. Pearce (67) points out that balanced incomplete block designs where only a few of the treatments are given to each subject are superior to orthogonal designs where all treatments are given to each subject, if it is desired to analyze out residual effects. If the occurrence of one treatment affects another given to the same subject it is possible to evaluate the effect. Pearce gives a worked example with and without interactions between remote and local effects. If the promise of this design is fulfilled for psychology, the psychologists will, fortunately, be spared the problem of recovery of interblock information, because it is precisely the distinction between intrablock and interblock comparisons which turns out to be the virtue rather than the vice of this design in this particular application.

Another approach shows that the residual effects problem as it occurs in the Latin square design may be specified rather completely. Ordinarily textbooks give as the mathematical model for the Latin square the following equation:

$$Y_{ijk} = \mu + r_i + c_j + t_k + e_{ijk}, \quad [2]$$

where Y_{ijk} is the score of the k th treatment in the i th row and the j th column, μ is the general mean, r_i is the row effect, c_j is the column effect, t_k is the treatment effect, and e_{ijk} is the normally distributed random error. Since the standard model provides for no interaction among the components, there has been some difference of opinion as to what might happen to the F -tests in experiments where interactions are actually present. Gourlay (38) showed previously that F -tests may be negatively biased in this situation, but that the presence of certain interactions need not interfere with the accuracy of analysis. Wilk & Kempthorne (90) have extensively examined the problem of nonadditivity in the random model Latin square design. They use as the mathematical model:

$$Y_{ijk} = \mu + r_i + c_j + t_k + (rc)_{ij} + (rt)_{ik} + (ct)_{jk} + (rct)_{ijk} + e_{ijk}, \quad [3]$$

where the double and triple subscript entries represent interactions between rows and columns, rows and treatments, columns and treatments, and rows, columns, and treatments. Using a randomization procedure, Wilk & Kempthorne find that the expectations of the mean squares show that the Latin square F -tests will not be positively biased as Neyman and his collaborators had concluded, but that the reverse will ordinarily be true. The authors show where Neyman went astray. Table 1 gives a comparison of the expected values of the mean squares in the textbook additive model situation and in the Wilk and Kempthorne nonadditive model. Examination of the expectations shows that the treatment mean square, when compared with the error mean square, will normally give a negatively biased F in the presence of interaction. If the row and column effects are considerable, the

TABLE I

COMPARISON OF EXPECTED VALUE OF MEAN SQUARES IN THE LATIN SQUARE
EXPERIMENT ($t=R=C=T$) IN THE CASE OF THE ADDITIVE AND NON-
ADDITIVE MODEL OF WILK AND KEMPTHORNE

Mean Square	Expectation with additive model (Equation 2)	Expectation with nonadditive model (Equation 3)
Rows	$\sigma^2_r + t\sigma^2_r$	$\sigma^2_r + \left(1 - \frac{2}{t}\right) \sigma^2_{rci} + \sigma^2_{ci} + t\sigma^2_r$
Columns	$\sigma^2_c + t\sigma^2_c$	$\sigma^2_c + \left(1 - \frac{2}{t}\right) \sigma^2_{rci} + \sigma^2_{ri} + t\sigma^2_c$
Treatments	$\sigma^2_t + t\sigma^2_t$	$\sigma^2_t + \left(1 - \frac{2}{t}\right) \sigma^2_{rci} + \sigma^2_{rc} + t\sigma^2_t$
Error	σ^2_e	$\sigma^2_e + \left(1 - \frac{3}{t}\right) \sigma^2_{rci} + \sigma^2_{rc} + \sigma^2_{ri} + \sigma^2_{ci}$

authors point out that the overestimation of the error term is unimportant. This would normally be the case when individual differences appear as the row effect in the Latin square. Although the Wilk-Kemphthorne treatment is in terms of random effects throughout, the insertion of one or more fixed effects will not eliminate the bias for all tests, and it is the fixed or mixed model that psychologists usually find appropriate. Unfortunately, the authors do not treat these two models, but the changes in their expectations are obvious. The authors point out that the Latin square design is much more efficient than the straight orthogonal design if there is considerable row and column variation.

Experimenters who seek balanced Latin square and related designs where the treatment sequences are arranged so that each treatment appears a certain number of times following another treatment, will appreciate the efforts of Sampford (70) who gives general procedures for generating such experimental designs.

Multiple range tests.—Most psychologists are now too sophisticated to use simple *t*-tests for examination of differences between means after an over-all analysis of variance. They will usually prefer to use some kind of multiple comparisons test, but they may be puzzled as to which test is best for their purposes. Characteristics of these tests are described by Federer (29). Although multiple range tests are difficult to compare, Federer indicates that the Duncan multiple range test (25) is unusually powerful against Type II errors. This also appears to be the conclusion to be derived from Harter's (44) study of error rates and sample sizes for various range tests used in multiple comparisons. Since the original Duncan multiple range tests were set up in terms of homoscedasticity and equal *N* per mean with

no provision for covariance, users of tests will be pleased to note that Kramer (56) and Duncan (26) have developed means of extending the tests to those annoying instances where variation in sample size, heteroscedasticity, and group-correlated adjusted means appear. The papers cover similar situations, but Duncan's procedures although a little more complicated seem also to be more adequate.

Analysis of covariance.—The 1957 volume of *Biometrics* contains three especially valuable papers on analysis of covariance. Cochran (13) and Smith (75) give concise, authoritative summaries of the purpose, function, procedures, and interpretations of analysis of covariance, and Coons (15) shows how the analysis of covariance may be used as a missing plot technique. Missing plots are not too common in psychology, but the missing plot analysis can often be used to check on the significance of a single aberrant observation, following procedures that are available in the standard textbooks. Smith's (76) multivariate analysis of covariance may also prove interesting, and his exploration of a number of problems in interpreting regression analyses points out many pitfalls in the interpretation of adjusted means.

Statistical testing procedures.—Papers covering a number of specialized statistical testing techniques have appeared during the year. For example, Foster & Rees (36) and Foster (35) have published tables of the upper percentage points of the generalized beta distribution which will be useful in multivariate analysis. And one of the newer techniques of analysis and multivariate regression theory, contour analysis, which is well described by Cochran & Cox in their second edition of *Experimental Designs* (14) is shown by Maxwell (60) to have a number of applications of particular interest to psychologists.

Stuart (78) gives a very simple method for comparison of frequencies in matched samples for fairly large-size N . The test is a generalization of the conventional, normal approximation to the difference between two proportions. Two samples are composed of matched pairs of subjects, one member of each pair in each sample. The matching need not be perfect. If S is the number of pairs who share the attribute to be tested, and if a is the number in the first sample having the attribute and c is the number in the second sample, then:

$$Y = \frac{|a - c| - \frac{1}{2}}{\sqrt{a + c - 2S}}, \quad [4]$$

is the unit normal deviate. Stuart seems unaware of McNemar's 1947 formula, which appears in American texts. The old chi-square goodness-of-fit test for normal distributions has been examined by Watson (87). The exact distribution of the common chi-square approximation:

$$\chi^2 = \sum \frac{(f_0 - f_i)^2}{f_i}, \quad [5]$$

where f_o is an observed frequency, and f_t is an expected frequency, is indeed asymptotically distributed as chi-square if the frequencies are efficiently estimated. At least 10 class intervals, however, must be used in practice so that the tabular points of the chi-square distributions are accurate to within 1 per cent.

Fitzpatrick (33) has critically reviewed the methods which have been used to detect differential accident susceptibility. Accident distributions resemble both the Poisson and negative binomial curves. The two distributions are hard to differentiate statistically, and may be generated by at least two conflicting models. In general, Fitzpatrick shows that univariate study of accident distributions will almost inevitably lead to inconclusive results. Bivariate methods, involving two or more periods of time, do very little better. Fitzpatrick points out that investigations of time periods between successive accidents may produce promising results. The situation remains difficult.

Social psychologists may have especial interests in two recent studies. Ferris (30) has described a method of analyzing consumer preferences by the "K-visit method." The techniques of estimation and testing involve the formation of diads and triads of preferences and reversals of preferences on the successive visits. More complete analysis of information contained in "ties" and reversals is possible with this method. Sociograms and communication networks in groups have a number of probability distributions associated with them which are analyzed by Katz & Powell (51). Examples are given on the distribution of indices of "gross expansiveness," equal to the total number of choices made by a group divided by the size of the group, and the distribution of "isolates" in a group. Katz & Powell note that neither of the distributions mentioned has been given correctly in the literature.

Analysis of Curves—Rao (68) has developed a very simple statistical method for comparing the rates in growth curves which may be applied in many exponential situations. Learning curves, adaptation curves, and the like can be analyzed by this method. The procedures are potentially more appropriate and useful than those involving orthogonal polynomials. Incidentally, Bahrick, Fitts & Briggs (4) show how all learning curves—and by implication other such functions—vary markedly in shape and apparent interpretations, depending upon choice of scoring systems, etc. Further techniques for analysis of psychometric functions are reported by Bross (7) for the case in which three or more response categories, such as, "Yes," "?," "No" are permitted.

NONPARAMETRIC STATISTICS

Nonparametric tests are relatively easy to devise, but their general properties are often difficult if not impossible to determine. The difficulties arise because these are tests of departure from randomization, and such departures may occur in an infinity of ways. A given nonparametric test must usually be more sensitive to some departures than to others. Because

the specific modes of departure can rarely be specified properly, alternative hypotheses cannot be scaled, and it is often impossible to speak sensibly of the "power" of a nonparametric test. Mathematical statisticians are attacking the general problem but it is impossible for the reviewer to assess their progress. Individual studies tend to be limited in scope and conclusions are hard to reach. For example Fieller, Hartley & Pearson (31) have compared the Spearman rank difference correlation coefficient with the Kendall tau in an empirical study which gives general indications favoring Kendall's tau.

Some much-needed negative thinking has recently appeared on nonparametric techniques. The searching, critical review of Siegel's lucid *Nonparametric Statistics for the Behavioral Sciences* (74) by Savage (71) examines the whole philosophy of nonparametric testing in general as it scrutinizes Siegel's book in particular. It deserves thoughtful study. Among other things Savage mentions casually what, in the reviewer's opinion, is the fundamental weakness of nonparametric techniques applied to scientific work. Most of these techniques are aimed exclusively at achieving decisions, and their structure may even make impossible the estimation of the magnitude of experimental effects. Savage is also less impressed with the power of nonparametric techniques than is Siegel. This last point is illustrated also by McNemar (62), who gives convincing evidence of the lack of power of Wilson's distribution-free test of analysis of variance hypotheses (91). Sheffield (73) attempted to make the same point but improperly used an interaction error term in a fixed model illustration.

Myers (65) calls attention to an exact probability treatment for factorial designs that will be useful with relatively small samples. The procedure is applicable when the subject's responses are dichotomized so that the data form essentially a multi-dimensional contingency table. This type of table occurs often and the exact technique can supplant the misuse of analysis of variance here.

PSYCHOLOGICAL SCALING

Three new scaling techniques have appeared. One is a new method for locating a rational origin for subjective preferences, reported by Thurstone & Jones (81). This was evidently one of the last projects that Professor Thurstone completed before his death. The procedure is rather similar to an earlier one of Horst, except that instead of including both desirable and undesirable stimuli, desirable stimuli only need be used. The critical assumption is that under some circumstances subjective values can be additive, so that the subjective value of a combination of two objects very closely approximates the sum of the subjective values of the objects considered singly. When this assumption holds, it is possible to obtain an estimate of a zero by considering the subjective estimates of all objects singly and by pairs. The additivity assumption is shown to work out well in placing money values on birthday gifts singly and in pairs. Another procedure for deriving equal discriminability scales from absolute ratings is reported

by Attneave & Chambliss (3). The procedure eliminates irrelevant components arising from consistent differences in the rating tendencies of individual judges. The method is very simple, and the results appear from the illustrative data to be highly reliable and to correlate well with graded dichotomies scale values. The third new technique involves a revision of the law of comparative judgment by Harris (43). A comparison pair and its complement are assumed to give rise to two different distributions of differences. An iterative technique is required for the solution of the scale values, and the procedure can be applied to the method of successive intervals.

Gerard & Shapiro (37) have developed some interesting new measures of the degree of inconsistency in a set of paired comparisons. The method applies when the rater is to make choices between pairs of objects that are supposedly simply ordered with respect to his own personal reference point on the scale. In a very elegant treatment two fundamental types of inconsistency, "intransitivity," which has its conventional meaning, and "separation," which implies rating a stimulus so that it has two or more positions on the continuum, are defined. It is then shown that an answer matrix is consistent if and only if it contains no intransitivities or separations. Simple formulae are derived to give the number of intransitivities in an answer matrix and the number of separations. Other more traditional methods of evaluating paired comparison data are examined by Jackson & Fleckenstein (50) who find that the scales resulting from the different methods are almost identical. Specific properties of the Thurstone-Mosteller, Scheffé, and Bradley-Terry methods are considered.

Thurstone's absolute scaling rationale has inspired a number of further attempts at simple methods of solution for the method of successive intervals. Diederich, Messick & Tucker (23) and Bock (5) present least squares solutions. The former, a modified Gulliksen procedure, is simple, but Bock's least squares solution is quite another matter. Bock presents an illustration comparing his solution favorably with the modified Gulliksen procedure. Thurstone's absolute scaling method has been shown by Chung-Teh (12) to lead to definite discrepancies between its estimates and the correct values when applied to item difficulty. The discrepancies are shown not to arise from sampling variation.

PREDICTION AND CORRELATION

General.—Relatively little original work appears to have been published in the area of correlation during the past year. In fact Edgerton (28) managed to publish Kelley's (53) 1923 formula for the average intercorrelation between series of scores whose means and standard deviations are equal, and he did not give it in the form handiest for ranks either. Cureton (22) has cleared up an erroneous paradoxical conclusion of Meehl & Rosen (63). The latter claimed that a predictor having substantial validity may be worse than useless in predicting a dichotomous criterion when the criterion characteristic occurs with very high or very low frequency. The difficulty

turns out to be the choice of a nonoptimal cutting score by Meehl & Rosen. Cureton shows by means of a diagram how the choice of a proper cutting score depends upon the base rate of frequency of occurrence of the criterion. Reanalysis of the Meehl-Rosen data reveals a gain rather than a loss in accuracy of prediction when the predictor is used.

The standard personnel classification problem can be solved for the static situation, but in practice assignment is likely to be sequential. Ward (86) develops a disposition index, DI, which is the average of all possible assignment sums if a particular assignment is made for a particular individual. Assignment of each individual according to the highest DI permits very nearly optimal sequential assignment.

When researchers must attempt to predict a criterion from a mixture of continuous scores and classifications, for example, marital status, they may often hit upon the technique of dummy variables. They consider, say, single, married, divorced, and widowed as four separate variables, and score the individual with a unity for one of the variables and zero for the other three. The four variables are then entered into the prediction equation along with the other items. This will ordinarily lead to indeterminacies in the optimum solutions for the regression weights and frustration in the researcher. Suits (79) points out the two simple alternative remedies to this problem. One procedure is simply to drop the constant term from the regression equation so that it becomes homogeneous, and the other procedure is to drop one of the dummy variables. The total amount of information is the same, and a determinate solution then becomes possible.

Computational methods.—Kendall's tau coefficient for rank correlation is often a bother to compute. This is particularly true in the case of tied ranks. Griffin (39) provides a graphic means of calculating tau which has the advantage of great simplicity. Cartwright (10) corrects an error in the equation for the variance of Kendall's tau given earlier by Schaeffer & Levitt (72).

Four publications include improvements in and modifications of the procedures for calculating multiple and partial correlation and regression constants. These include a new book on multivariate correlational analysis by DuBois (24) and papers by Foote (34), Cowden (17), and Kramer (55). Foote avoids the back solution, Cowden provides for direct computation of regression weights, and Kramer makes two simple innovations that are convenient in the calculations.

FACTOR ANALYSIS

The most general current treatment of factor analysis is by Henrysson (46) who has presented an excellent up-to-date summary of factor history, current methods, and possible applications to a variety of problems in the behavioral sciences. But the most ambitious advances in the theory and practice of factor analysis may be attributed to Ahmavaara (1) and Tryon (82). Ahmavaara, after reformulating Thurstone's older factor theory of

mind in terms of linear vector fields, goes on to the original second part of his monograph which is devoted to the development and application of "transformation analysis." This procedure may permit the comparison of different factor studies with each other in a very fundamental way. Essentially the factors in one group of subjects are transformed by linear equations into the factor space of some other group of subjects. It is then possible to study the intercorrelations of the factors when the group has been changed. A large number of comparisons are made of factors relating to reasoning, verbal ability, musical ability, and mechanical ability. Ahmavaara feels that his results provide evidence for the additive model as a good approximation to a description of mind. Tryon (82) gives three methods for obtaining the "exact" communalities by iterative procedures. He further gives five methods of getting approximations to the exact diagonal values so as to initiate the iterative process. In an illustrative 10-variable problem programmed on the IBM type-650 computer, iterations required only five minutes and, with the trial values taken, convergence was so rapid that the third or fourth iterations were virtually exact. Tryon's Key Cluster method permitted the excellent first approximations in an area where convergence has been so slow as to have discouraged previous users of iterative procedures. Tryon's procedure is impressive with the illustrative problem. But that problem consisted of fictitious data, and the success of his method with live data awaits further research.

Other contributions extend the frontiers more modestly. Creager (18) has generalized the derivation of multiple and partial regression statistics from factor loadings to the generalized oblique solution. Tucker (84) has given preliminary consideration to the problem of the determination of parameters in nonlinear functional relationships between variables by means of estimation from factor analysis. The method might, for example, be applied to the parameters of a Taylor's series or a Fourier series. An approximate algebraic method for factor rotation aimed to maximize agreement's between loadings and predetermined weights is presented by Rodgers (69). This method might be used in Eysenck's criterion analysis or in any other hypothetico-deductive approach.

Guttman (41) gives another of his famous simple proofs, that for the relationship:

$$R^2_{i,n-1} \leq h^2_i, \quad [6]$$

for all i . Where $R^2_{i,n-1}$ is the multiple correlation between the i^{th} variable and the remaining ones, and h^2_i is the communality of the i^{th} variable. Guttman shows that the inequality holds for a variance ratio, η^2_i , as well as for the communality. In another comment on the communality problem, Wrigley (92) claims that psychologically more reasonable diagonal entries would be the squared multiple correlations rather than the communalities, which shift from test battery to test battery. The multiple correlations obviously suffer from the same defect, but Wrigley is on firmer ground when he recommends that reliability coefficients be placed in the diagonals where

common variance analysis is the interest rather than factors in the classical sense. Finally, the "factorists" as Tryon calls them will scarcely agree with Hotelling (49) who claims that modern multivariate techniques can handle all factor analytic problems better than factor analysis itself and that factor analysis has only heuristic or suggestive value, or with Creasy (19) who regards analysis of variance as an alternative superior to factor analysis.

PSYCHOLOGICAL TESTING

In his Presidential Address to the Psychometric Society, Brogden (6) emphasized that consideration of the actual application of the product of testing research may often lead to useful reformulation of old problems. For example, partial regression weights yield a composite score which will correlate maximally with a criterion, whereas the least squares method designed to yield a composite that has maximum correlation with the true composite will be biased for validation purposes.

In the general field of psychological testing numerous limited advances were reported. Horst & MacEwan (48) have extended the method for obtaining optimal test length to situations where the altered time allotment for one or more of the tests may approach zero. McHugh (61) provides an astonishingly simple formula for the number of cases required in a validation study where a specified degree of accuracy of the validation coefficients is required. One of his simple formulae is:

$$n_1 = 3 + \frac{Z^2_{\alpha/2}}{d^2_r}, \quad [7]$$

where $Z^2_{\alpha/2}$ is the normal deviate corresponding to the alpha risk level, and d^2_r is the permissible deviation from the true validity coefficient. In a problem related to the inverse of that of Horst & MacEwan, Hoffman (47) derives equations relating the length of the test to its weight in a composite. Weighting schemes involving standard deviations of raw scores and standard deviations of true scores are compared, and it is shown that the method works with actual tests of varying length. An important secondary result is a new derivation which estimates the reliability of a shortened test, knowing only the relative length of the test and the standard deviation of both lengths. And Lubin (58) classifies predictors as valid, suppressor, negative suppressor, and invalid. He presents a number of formulae for use with suppressor variables which are designed to indicate the kinds of predictors which should be constructed and the results which may be expected from their use in the battery.

Item and composite scoring.—The problem of item scoring and combination into composites has received a good deal of attention this year. MacLean (59) shows how the item-score matrix, usually available on a set of punched cards, may be used directly to produce the various composites of weighted items, phi coefficients, and the like. Several of the other reported methods represent formidable amounts of work. For example the iterative scheme for deriving a composite score from several measures of the same

attribute, developed by Dunnette & Hoggatt (27), follows the approaches of Horst and Wilks. Osburn & Lubin (66) show how to obtain an exact statistical test of whether a selected scoring technique has optimum validity by considering the configurations or patterns of item answers. Their method becomes virtually impossible with more than about five items in the test. A somewhat similar approach is used by Brownless & Keats (8) who propose retesting a given individual and examining the frequency of all possible pairs of responses to each item in order to correct for partial knowledge. This is possible; its desirability remains to be seen. In the problem of converting scores on one form of a test given to one group of subjects to those on another form administered to another group in cases where the two forms have a common subset of items, Swineford & Chung-Teh (80) provide a rather simple item method of conversion which they claim gives highly satisfactory results, particularly when the two groups of subjects are rather different.

Some attention has been given to measuring or partialing out response set from the true score. Helmstadter (45) gives two models by which response set can be separated from content components in a test score. Response set is the tendency for a person to give a different response on a test item from the one he would give if the same content were presented in different form or context. Helmstadter's method applies only to dichotomous items. In contrast to Helmstadter's approach, Webster (89) used a correlational procedure to obtain a score uncontaminated by set, and he shows how the elimination of set components will usually reduce the reliability coefficients on tests such as structured personality scales.

RELIABILITY

The most interesting contribution to the understanding of reliability and the computation of reliability coefficients comes from Tryon (83) who points out that the classical Spearman approach is unnecessary, as all modern formulae can be derived from simpler assumptions that are more closely tied to the operations of psychological testing. All the standards procedures for computing reliability coefficients are related to his behavior domain formulation, and a "new individual variance form" is presented for the calculation of r_{tt} . This new form yields the same estimate as Hoyt's 1941 variance formula. The paper closes with an enthusiastic section labeled "History of Orthodoxy in Mental Test Theory." Tryon's paper should have considerable impact on thinking and practice regarding reliability, although he points out that Gulliksen (40) and others had previously broken from orthodoxy. Another important paper by Cotton, Campbell & Malone (16) shows that when dichotomous test items are used Cronbach's (20) notions require reanalysis, because the factor structure of the continuous case does not exist for the dichotomous item distributions.

Lord (57) has provided a maximum likelihood test for the hypothesis that the correlation between two variables is unity except for errors of measurement. The procedure is difficult, and no exact sampling distributions

are given, but the asymptotic chi-square relation is presumed to hold. Finally, in a re-examination of Kelley's old 27 per cent rule Cureton (21) shows by a simple derivation that although it holds for normal distributions it should be 33 per cent for rectangular distributions. Webster (88) provides some simple item-counting procedures which will maximize obtained homogeneity as measured by the Kuder-Richardson Formula 20.

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PROBLEM SOLVING AND THINKING

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The trend of increasing interest of psychologists in problem solving, thinking, and related processes, remarked by Taylor & McNemar (88) in their earlier review, appears to have continued and accelerated during the period covered by this review (April 1954 to April 1958, approximately). There appear to be the beginnings of a solid experimental literature on this very old psychological topic. The serious student who enters the maze of intellectual functions, if he is able to become negatively adapted to the philosophical noise which still persists, learns to thread his way through the obfuscatory traps of terminology, and overcomes the temptations of goal-oriented mathematical cul-de-sacs, will obtain at least substantial aperiodic reinforcement from this literature.

A reviewer has many decisions to make in determining what to include in such an article as this. Thinking, problem solving, concept formation, and decision making are important behavioral functions which impinge upon nearly every other area of psychology. For reasons that are primarily expository, I have chosen to review that body of literature which in some small or large way adds to our fundamental understanding of how people think. Thinking is a general term referring to the occurrence of central events which are not simply initiated by the specific stimulus situation in which the individual finds himself, and which are in part directly experienced as "thoughts," "ideas," "images," etc. In this general sense, thinking includes the kinds of phenomena referred to variously as ideation, imagining, and daydreaming. However, for the purpose of this review, we shall confine ourselves to the kind of "productive" thinking called problem solving.

Assuming that the individual is motivated to achieve some definable, and also attainable, goal, problem solving occurs when the individual cannot reach that goal by means of behavior based simply on reflexes or habits. He must, in fact, use a mediating process involving concepts, which are most readily thought of, in adult human beings, as words. It seems evident that concepts must have as a bare minimum the properties described for the symbolic process by Hunter (46), the fractional goal response by Hull (45), and "learning set" by Harlow (40). In addition they possess the extremely powerful properties of generalization that words are known to have.

Problem solving begins with a stimulus situation, and instructions which establish sets and define the goal. The behavior is exhibited in a number of phases, which may be conceived as follows. Phase one is the reception of the stimulus situation. Sometimes, the situation is entirely apparent to the subject from the beginning; or, it may be presented in a sequential manner;

or again, the subject's responses may change the situation as he proceeds. Concept formation, or concept invention, is a second phase, which probably has its basis in transfer of training. This behavior is greatly influenced by strategies, that is, by rules that subjects adopt (whether or not they can state them clearly) to determine which parts of the stimulus situation to react to. Some kinds of problems (like trouble shooting, solving mathematical problems) appear not to require concept invention.

Determining courses of action is a third and central phase in problem solving. The courses of action available to the subject may be few to very many. This phase is also influenced by strategies, which may be relatively "conservative" or relatively "risky," and which interact with the concepts of outcome probabilities the subject has. Decision making may be distinguished as a fourth phase of problem solving. It is a process of choice or judgment that occurs when two or more courses of action are known to the individual, each of which provides an "adequate" solution. Verification is the final stage. Here the research problems concern how the activities of problem solving are affected by feedback from the accomplishment of a solution, or from the discovery that an attempted solution is wrong.

Only infrequently do psychologists include all these phases in their studies of problem solving, and most often only a single phase is studied as a focus of interest. Because some phases have not been studied systematically, the topics of our review cannot follow such an outline exactly. Instead, it will deal respectively with the topics of (a) general treatments of problem solving, including articles of theoretical interest; (b) studies of concept formation; (c) research on problem solving with emphasis on the choice of courses of action, including investigations of trouble shooting; and (d) studies of decision making which are judged to make significant contributions to the psychology of thinking.

WORKS OF GENERAL IMPORTANCE

A number of books and articles have general importance for research on thinking, either because they provide basic theoretical frameworks, or because they draw together and interpret results from widely scattered sources.

Theory.—A book of considerable significance to research and theory is *Contemporary Approaches to Cognition* (5), the report of a symposium held at the University of Colorado. Brunswik's presentation (pp. 3-32) emphasizes the view that both organism and environment have to be treated as interacting systems, each with properties of its own. The full scope of the cognitive problem is conceived as involving the entire set of variables from the distal stimulus through the proximal sensory inputs to the central process. Three levels of problems are distinguished: (a) cognitive achievement, the correspondences between distal and central variables; (b) the strategy of peripheral mediation between distal and central variables; and (c) mediational tactics, or the micromediation performed by central processes. Brunswik further discusses these distinctions in relation to a number

of well-known approaches to the study of perception. Bruner (pp. 41-69) discusses the topic "Going Beyond the Information Given." The forms of behavior exhibiting this characteristic range all the way from identifying objects to scientific theory construction. According to Bruner, the organism performs in these ways by the use of coding systems. A coding system "is the person's manner of grouping and relating information about his world, and it is constantly subject to change and reorganization" (p. 46). The problems of cognition are considered to be those of (a) the conditions of acquiring coding systems; (b) the invention of highly generic codes; (c) the invention of ways of utilizing codes; and (d) the problem of how to instruct people in codes so as to guarantee maximum ability to generalize. Bruner then discusses a number of factors which, according to previous evidence, have important effects on each of these functions. Osgood (pp. 75-118) gives "A Behavioristic Analysis of Perception and Language as Cognitive Phenomena," a stimulus-response model of behavior with emphasis on mediating processes. First of all, he distinguishes two basic types of behavioral processes, "decoding" and "encoding." By the former is meant the process whereby the physical energies of the environment are interpreted (i.e., organized into various kinds of internal events) by an organism. Encoding is the process whereby the organism expresses these internally organized events. For both decoding and encoding, there are three "levels" of organization: (a) a "projection level," which relates both receptor and muscle events via neural mechanisms which are not modifiable by experience; (b) an "integration level," which is exemplified by certain perceptual phenomena, simple habits, and motor skills; and (c) a "representation level," the realm including meaningful language and concepts. When one undertakes to index these representational processes by means of independent measures, he undertakes to measure "meaning"; and this is what Osgood and his associates have done by means of the semantic differential technique. Festinger (pp. 127-150) examines some relations between "cognition" and behavior, employing the basic hypothesis that there exists a tendency to make one's behavior consonant with one's cognition. He describes a number of studies in which values (or attitudes), as measured by choices, may be altered by environmental circumstances to bring about increased dissonance between the attitude and the behavior, and thus greater pressure to reduce this dissonance. Finally, Rapaport (pp. 157-200) describes certain "cognitive structures," by which is meant the persisting "tools of cognition" rather than the temporary "processes." Drawing upon varied sources, including clinical observations, Rapaport illustrates three examples of these cognitive structures: (a) memory organizations, including Bartlett's schemata; (b) grammar and syntax in language; and (c) styles, i.e., styles of perceiving, conversing, dreaming, and so on. Heider (pp. 201-210) provides an integrative summary of the views represented in the symposium. As a whole, the book is a provocative source of ideas for research on thinking and related types of behavior.

The book *The Measurement of Meaning* [Osgood, Suci & Tannenbaum

(70)] is a systematic account of a method of measuring mediational processes and its application to the use of concepts, the assessment of attitudes, and communications research. By meaning the authors mean an internal process which operates in the reception of stimuli as signs and in the production of sign-responses. This mediating process is defined as follows (p. 7):

A pattern of stimulation which is not the significate is a sign of that significate if it evokes in the organism a mediating process, this process (a) being some fractional part of the total behavior elicited by the significate and (b) producing responses which would not occur without the previous contiguity of non-significate and significate patterns of stimulation.

Meaning is measured by means of a method called semantic differentiation. Subjects are asked to rate concepts on seven-point scales referring to polar adjectives such as good-bad, strong-weak, active-passive. The meaning of a concept to an individual is measured as the set of scores on each of these scales. The major implication of these ideas for the study of thinking appears to be that a method is provided for sensitive detection of the direction and strength of mediating processes which are not, or perhaps cannot be, explicitly verbalized.

Skinner's stimulating account of verbal behavior (82) distinguishes a number of types of "verbal operants" which are considered to be accounted for in terms of the antecedent conditions of the stimulus situation and reinforcement schedules. There is the *mand* (example, "Wait!"); the *tact* (sometimes called a "sign" or "symbol"); *echoic*, *textual*, and *intraverbal* operants; and *autoclitic* responses (modifiers which express the intention of the speaker or qualify the listener's responses). Activities usually called "thinking" occur when the speaker and listener are within the same skin. "Covert verbal behavior" is a useful phrase for much of this activity, but little is gained by identifying thinking and covert speech, and certainly the notion that covert speech precedes or causes overt responses must be rejected. Verbal behavior has a number of important practical effects on the speaker as a listener. A self-mand, like "Get up!" may help to get the individual out of bed. A self-tact helps the speaker to clarify the situation to which it is a response, by categorizing, and also may function as self-instruction. In problem solving, the speaker generates stimuli to supplement other behavior already in his repertoire; the speaker may "tease out" (invent?) an effective classifying response. All the important properties of these types of behavior are to be found, Skinner says, in verbal systems composed of separate speakers and listeners. Few psychologists of behavioristic bent will find difficulty in accepting, at least contingently, Skinner's view that thinking occurs when the individual functions as both a speaker and a listener to his own verbal responses. Although it is apparent that Skinner would consider the use of inferred mediating processes superfluous, it seems probable that two such terms as "concept" and "self-tact" lead to sets of experimental operations that are not very different.

General treatments.—Johnson's textbook (52), *The Psychology of Thought and Judgment*, is a comprehensive treatment of the subject, including the historical developments which have influenced modern views. Johnson deals with attention, perception, and learning as topics of relevance to thinking, as well as concept formation and the solving of problems. Judgment is considered a process subsuming the findings of psychophysics, attitude measurement, and decision making. A summary of factors identified by studies of individual differences with factor analysis methods is also included, as is material on intellectual activities in social groups.

Karwoski (53) presents a systematic treatment of cognitive processes, including sensation, perception, memory, and thinking, which have the common function for the individual of "representing reality." Perception is conceived as reaction to signs (cues) which derive from objects and the environment. Thinking, on the other hand, utilizes symbols, which represent things in the environment. Karwoski thinks of mediating processes as essential to both perception and thinking; however, the properties of the mediators are different in these two processes. This is in contrast to the views of some authors, who appear to equate the mediating mechanisms in perception and thinking when they speak of "hypotheses" or of "assumptions." Scheerer's (79) treatment of cognitive theory discusses the issues raised with respect to perception and thinking by differing major theoretical formulations, particularly Gestalt theory and stimulus-response theory. Illustrations of critical issues are given in terms of the molar-molecular controversy, the relation of motivation and cognition, the development of intelligence, and the use of explanatory mechanisms applicable to simpler animal behavior ("genetic reductionism").

Hearnshaw (42) briefly reviews some important historical events in the study of conceptual thinking, and identifies five problem areas for research: (a) levels of complexity of thinking and their relation to phylogenetic development; (b) neurophysiological mechanisms; (c) the functional significance of conceptual development; (d) the place of conceptual capacity in the structure of human abilities; and (e) factors producing breakdown in the capacity for conceptual thinking. A summary of descriptions of the process of thinking in terms of the "stages" of preparation, incubation, illumination, and verification or revision is given in a book by Patrick (72).

CONCEPT FORMATION

Conceptual development.—There is continuing interest in the development of concepts in children as a function of age, and in the processes underlying such development. Using his highly developed method of systematic observation of children, with none but the simplest of props, Piaget (73) describes the critical behavioral events which distinguish stages in the child's development of concepts of objects, the spatial field, causality, and the temporal field. Concerning objects, for example, children's behavior is said to follow six developmental stages. In the first two, the child first

recognizes (i.e., differentiates), grasps, and then actively searches for an object, but shows no behavior directed toward vanished objects. In the third stage, he removes solid objects which may mask the desired object. The fourth stage is characterized by active search for a vanished object, but at the same time an inability to take account of the sequence of displacements which have led to its vanishing. The latter events are taken into account in the fifth stage. Finally, in the sixth stage, the child can find objects when their displacements have been invisible. When he does this, he must use a representational process, Piaget believes; in other words, he has acquired a concept of a permanent object with an existence separate from his own overt behavior. Similar analyses, accompanied by carefully recorded descriptions, are provided for the development of concepts of groups and the spatial field, objective causality, and objective time. In this book we have a theory that is tied at every point to careful behavioral observation. In general, these observations can readily be transformed into experimental operations, should anyone wish to undertake more extensive or precise verification.

Strategies in concept formation.—A major work in this area is a book by Bruner, Goodnow & Austin (6), which describes a group of studies designed to explore the behavior involved in concept invention. The basic materials for these studies were a set of cards containing figures which could be categorized in various ways. In different experiments, subjects might be asked to form categories which were conjunctive (the joint presence of several attributes, such as all cards containing two yellow rectangles); disjunctive (e.g., possessing two yellow rectangles or any constituent thereof, such as two figures, yellow figures, or rectangles); or relational (defined by a specifiable relationship between attributes, such as cards having the two figures of a different color). Interest was centered on the type of strategy or strategies used by the subjects in concept attainment.

These investigators describe four ideal strategies, and the advantages and disadvantages of each. Simultaneous scanning means that the subject uses each instance (whether positive or negative) to deduce which hypotheses are tenable and which have been eliminated. To use this strategy efficiently, the individual must plan each step so that his next choice will be maximally informative. Furthermore, after each choice, he must remember which concepts have been eliminated, and carry this memory through to the end of the problem. Successive scanning consists in testing a single hypothesis at a time. After choosing a particular hypothesis (e.g., "all cards containing yellow figures"), he limits his choices to those instances that provide a direct test of this hypothesis. With this method, little material has to be carried in memory, thus reducing what the authors call cognitive strain. On the other hand, this strategy does not guarantee maximum informativeness of each choice. Nor does it enable the individual to regulate risk, that is, to take a gamble with the hope of obtaining greater information. Conservative focussing is a third strategy, consisting of finding a positive in-

stance of the particular concept, then making a sequence of choices altering one attribute at a time, to see whether each change yields a positive or a negative instance. This strategy has the advantage of low cognitive strain and high informativeness of each choice. It is not a "risky" strategy, since each choice is bound to have relevant information; on the other hand, each choice does not have the maximum information. Focus gambling is similar in its use of a positive instance as a focus, but the individual changes more than one attribute value at a time. By so doing, he takes a risk whenever the change yields a negative instance; but he may maximize the informativeness of the choice if the instance is positive. Cognitive strain is low in this strategy. A number of studies are reported concerning the conditions under which subjects adopt these different strategies, when dealing with conjunctive and disjunctive concepts, abstract and meaningful materials, and with probabilistic cues. On the whole, the book contains many new ideas presented in a framework of systematic thinking. It seems most nearly correct to consider the findings as relevant primarily to what we have called the second phase of problem solving, the formation of concepts.

The importance of the factor of stimulus sequence in concept formation was studied by Detambel & Stolurow (20), using sets of three rectangles of different heights. The subject's task was to form class concepts for these figures. It was shown that sequences which provide successive contrasts between relevant and irrelevant stimulus components were more effective for learning than those which do not provide such contrasts. The acquisition of concepts pertaining to sequential dependency of panel lights connected in sequences of 4, 6, 8, and 10 units in length was studied by French (27). The subject's task was to discover the correct sequence in which to depress a row of buttons in order to turn all the lights on, under conditions in which an improper sequence resulted in turning off lights which had been previously lit. Instructions in general principles and practice on them were found to have a significant effect in reducing error scores in the achievement of the required concepts.

Utilization of concepts.—Several studies on human concept formation have been stimulated by Harlow's ideas on learning sets (40). Adams (1) tested the hypothesis that individuals who have practiced responding to concepts defined by the same stimulus objects during 24 trials would acquire a new concept as well as individuals who practiced responding on the basis of 24 different stimulus objects. The group having "single" pair training exhibited more rapid learning than did the group having "multiple" training. Callantine & Warren (9) report results which imply a different conclusion, in an experiment on the learning of concepts from card figures differing in form and color. Their results showed that increasing levels of performance were related to increasing variation in the examples used to represent the initial concepts. One important difference between this and the Adams experiment was that a higher level of initial performance was achieved by the groups which had "variable" training. A somewhat related

study, this time varying number of concepts rather than number of stimulus situations, was performed by Semne & Warren (80), using two versions of the Wisconsin Card Sorting Test. Different groups received training with 0, 1, 2, or 5 shifts in concepts to be acquired, and a control group received no pretraining. The control and 0-shift groups made significantly more, and the 5-shift group significantly fewer errors on the final task than the 1- and 2-shift groups. The acquisition of a set to shift is suggested as an explanation.

The investigation of Kendler & Vineberg (55) sheds light on conditions of concept formation and use in human beings. Subjects first practiced sorting cards of the N.Y.U. Card Sorting Test (similar to the Wisconsin Card Sorting Test) into bins in accordance with a simple concept (size, color, shape, or "part-whole"); then they practiced sorting according to a second simple concept; finally they were tested in their ability to sort according to compound concepts made up of two simple ones (e.g., cards having figures which were of the same size and simultaneously the same shape). The results of two similar experiments were inconsistent, one showing significant effects on rate of learning the compound concept of number of appropriate simple concepts previously learned, the other insignificant effects. According to the authors, the difference may lie in the success with which subjects are able to verbalize the simple concepts. Kendler & D'Amato (56) performed three experiments to test further the hypothesis that implicit symbolic responses mediate overt card-sorting behavior in the formation of concepts. In the first experiment, subjects who had acquired a particular concept (shape or color) were then given the task of sorting cards to a reversal of this concept. Their performance was compared with subjects who had to shift concepts in sorting cards but not to reverse the originally learned concept, and with a control group which received no training on the first concept. The results showed that a reversal shift occurred at a significantly faster rate than did a nonreversal shift. The second experiment was designed to isolate the effect of number of shifts required of the comparison groups, the third to test the effect of equating for partial reinforcement effects. Kendler & Mayzner (57) showed that the relative effectiveness of a reversal shift over a nonreversal shift was lost when four categories of sorting were used rather than two, as would be predicted from the conditions of reinforcement of the mediating concept. The results of these studies provide evidence that the presence of appropriate symbolic cues (concepts) facilitates the sorting performance even when these cues have been originally connected to the "wrong" sorting responses. Thus they demonstrate the power of concepts to overcome the interference which takes place in reversal of sorting responses based upon the simpler mediating process of habit.

Heidbreder & Zimmerman (43) performed an experiment to determine the effect of the semantic efficiency of phrases on the attainment of concepts categorizing these phrases. At one level, semantic efficiency took the form

of using a noun to designate the critical feature; at a second level, an adjective designated the critical feature, and at the third level, neither adjective nor noun referred to the critical feature. The results show significant differences in trials required to attain concepts between the three levels of semantic efficiency. The study may be considered as providing additional evidence of the importance of verbal generalization in the attainment of concepts in adult human beings.

Underwood & Richardson (92) describe 213 nouns for use in the study of concept formation, with per cent frequencies of certain adjectives given as associations to these nouns by 153 college students. The associations to be obtained were restricted to "sensory impressions," and yielded 40 response categories like "round," "big," "greasy," etc. These nouns are suggested for use in studies of concept learning when some control over previously learned response tendencies is desired.

Comment.—The description of types of strategy in concept invention and some new methods of studying these strategies (6) is an important development. Further attention needs to be paid to the effects of systematic variation in methods of presenting the stimulus situation, a variable suggested by a number of these studies. Another promising approach appears to be the measurement of concept effectiveness by transfer methods, as suggested in the studies of Kendler and his associates (55, 56). A healthy sign in this area of research is the appearance of a number of new approaches, techniques, and materials.

COURSES OF ACTION IN PROBLEM SOLVING

Variables affecting problem solution.—A series of exploratory experimental studies on various aspects of problem solving has been collected and edited by Taylor & McNemar (89). In solving problems of arithmetic reasoning and spatial relations, performance was found to increase with the sizes of groups constituted of one, two, and four individuals (Taylor, Whipple & Carey, pp. 1-10). Announced interruptions of work during one-hour sessions were found to be ineffective in overcoming inappropriate sets in solving spatial and verbal problems (Irvine & Taylor, pp. 11-17). When subjects were allowed to choose their own time for breaks, achievement in solving anagrams appeared higher than that for a continuous work condition, but difficulties in controlling accurate timing make the results inconclusive (Taylor & LaBerge, pp. 18-23). An experiment on transfer in problem solving is reported (Milton, pp. 43-53), in which subjects were given initial training on problems of varying "breadth," selected in accordance with their factor loadings on analyses previously carried out by Guilford and his associates. Transfer was found only between problems which were highly similar. Another study (LaBerge, pp. 54-65) applied the concept of stimulus redundancy to the acquisition of concepts referring to nonsense patterns. The results, although inconclusive, suggest that redundancy interacts with distinctiveness in determining concept acquisition. The effect of

success and failure on problem solving was investigated (Taylor & Carey, pp. 66-72; Taylor, Way & Irvine, pp. 73-78; Rhine & Milton, pp. 79-87) in several studies. A suggestion of sex differences in the effects of these factors found in one study did not occur in a second. No confirmation was found for the hypothesis that subjects who experienced a high degree of initial success solved more problems than those who did not. No effects of failure to solve on posttest anxiety were found. Pretesting results are given (McNemar & Taylor, pp. 88-98) for a series of anagrams and spatial puzzles designed for use in studies of set.

Saugstad (76) examines Maier's experimental method in the two-pendulum problem from the standpoint of whether or not Maier's subjects had all the experience necessary for the solution of the problem. He reports an experiment using a modification of Maier's candle problem, in which a group of subjects were first asked to list all the "functions" they could think of for the tubes, rod, and putty used (later) in the problem. Subjects were then presented with the candle problem, and a comparison made of the proportion of solutions achieved by subjects who had stated at least two functions, and by subjects who stated less than two. Saugstad interprets his results to mean that solution of problems like these depends to an important degree on the availability of relevant concepts, and that "combining of experiences" may not be a factor of great importance (as Maier suggests).

The same author (77) also reports three experiments designed to provide tests of Maier's hypotheses derived from the two-pendulum problem, namely, that solution to the problem requires "direction" in addition to the "part experiences" provided preliminary to the problem situation. Saugstad proposes that the "direction" given by Maier may have served to draw attention to the ceiling for one group of subjects, whereas it may have been "unavailable" to others who did not receive "direction." The first experiment was designed to compare performance in a situation closely similar to Maier's, presenting the same problem in a miniature hallway (which made the ceiling a very apparent part of the situation). Half of each experimental group received "direction," half did not. The results indicated no significant effects of "direction" in either group. In a second experiment, he asked one group of subjects to perform preliminary tasks using part-principles, whereas the other group was merely given instructions for the problem as described by Maier. A significantly greater proportion of solutions was found in the group which had actually performed the three tasks based on part-principles. A third experiment was designed to test the effect of clarifying instructions on "Principle C" (pertaining to keeping a pole in a fixed position by pressing it between two surfaces). Again these instructions were found to be successful in increasing correct solutions. These studies suggest that the integration of concepts into a total solution may not be critically influenced by the factor of "direction."

Concerning the availability of concepts, Staats (85) sought for relationships between available verbal responses and solving of the Maier two-

string test. He first asked his subjects to list all of the different ways that each object might be used. In the following week, each subject attempted to solve the problem. No significant relationship was found between preproblem verbal responses and problem solution. A positive correlation was obtained between fluency of verbal responding and time to solve the problem. Kendler & Kendler (58) employed a simple problem situation, derived from Maier's work with rats, to test for inference in the sense of "combining experiences" in three- to four-year-old children who first learned three separate tasks of obtaining three toys by pulling strings. The experiment was designed to test whether the separate experience of pulling A to get B, pulling X to get Y, and pulling B to get G (the major toy) would be integrated in such a way that the children would pull A to get B to get G, in other words, would make the necessary combining inference. A highly significant proportion of the experimental group did behave inferentially, as compared with a control group of children who did not have the B-G experience.

Techniques, old and new.—Ray (74) provides a review of complex tasks which have been used in research on human problem solving. A problem is considered as divisible into the parts of (a) the given situation; (b) the desired situation; and (c) the method of proceeding from one to the other. Ray points out that experiments have used either of two procedures: they ask the individual to discover the operations leading to solution, or they provide the initial situation and the operations and ask him to predict the solution. Twenty-nine different tasks are described. The author considers several characteristics of tasks which can aid experimentation, including provisions for a continuum of scores, limiting the number of relevant hypotheses, using more than one score, making possible overt behavior, avoiding special knowledges. Various types of scoring procedures, yielding both quantitative and qualitative measures, are also discussed. The article has considerable value as a systematic reference to studies of problem solving.

Moore & Anderson (68) describe materials from the calculus of propositions and their use in studying problem solving. The content consists of (a) three kinds of symbols, namely, propositional variables, constants, and punctuation; (b) rules for formation and transposition of propositions; (c) a set of postulates and derived theorems; and (d) definitions. A number of advantages of these materials for investigations of problem solving are given; among these are the feasibility of systematic variation in complexity of problems, the recording of steps in solution, the possibility of introducing insoluble problems, and freedom of the materials from factors reflecting previous experience. It is evident that, when used in the manner described, these problem materials do not require concept invention on the part of the subject, but they appear to have many desirable characteristics for the study of courses of action in sequential tasks. Using problems derived from these materials, Anderson (2) studied the behavior resulting when more than one possible goal is set. Her hypothesis was that with more than one

goal, competing responses would have mutually interfering effects. Subjects were given problems to solve which, for the particular experimental condition, might have one, two, three, or four goals stated. Under the multiple-goal conditions, only one goal was attainable, but the subjects did not know this. The results of the study show that the probabilities of problem solution increase in a near-linear fashion with the taking of each relevant (i.e., correct) step. However, although trends appear in the data, no significant evidence was found for decrease in solution efficiency as a result of increasing the number of goals.

Battig (3) explored the use of a word-formation game in problem solving, pointing out the advantages of obtaining data on each step in solution (i.e., each letter supplied) by such means. Words of four to eight letters in length and of different frequencies of occurrence were selected for study. Significant variability in performance between words was found, much of which could be attributed to the specific letters contained in the words. Differences in strategy were found between the lower and upper quarter of subjects, divided according to their performance; the upper quarter tended to guess letters in the order of their frequency of usage, whereas the lower quarter tended to use alphabetical order. Battig believes this game has considerable promise as a means of studying problem solving, but emphasizes that its major shortcoming is the dependence of responses on the particular words used. Calibration of word difficulties appears necessary to provide sufficient standardization so that the method can have general usefulness.

Springbett, Dark and Clake (83) describe the Lines Test, which presents the subject with an immediate memory task composed of a series of figures drawn using the sides and diagonals of a grid, in three categories: (a) meaningful figures (e.g., a box); (b) Gestalt figures (displaying symmetry only); and (c) nonsense figures. In a second study Springbett (84) used a revised version of the Lines Test in which the lines formed (a) short words; (b) words with distorted letters; and (c) nonsense figures. In both these studies correlations with reference tests are reported. The Lines Test may be a useful instrument for measuring the action of conceptual processes, but its evaluation as a test would appear to require larger groups of subjects in a systematically-constituted reference battery.

Cross & Gaier (13) report a study on the use of "facts" and "principles" by subjects in solving problems involving the balancing of weights. The material is presented in a test requiring the pulling of tabs to disclose either a principle or a fact, called the Balance Problems Test. John & Miller (51) describe an ingenious task called the problem solving and information apparatus, which presents to the subject a circular array of lights on a panel. These elements are wired together in correspondence with logical relationships, and problems of varying degrees of complexity may be presented. A central light, representing the output of a network of elements, must be turned on by some combination or sequence of activation of three particular elements. Essentially, the subject "questions" the apparatus to determine the

rules to follow in turning on the central light. Various uses are suggested by the authors for this versatile apparatus.

Theory.—Maltzman (63) proposes a theory relating problem solving to the simpler processes of habit learning, by means of the concepts of the fractional anticipatory goal response and the habit family hierarchy. First of all, it is pointed out that a specific external stimulus (and accompanying internal drive stimulus) may become associated with a hierarchy of habit sequences, each of which has a different strength and all of which may be associated by previous learning with a mediating fractional goal response. The same initial stimulus may in turn become associated with other habit family hierarchies, through the different mediators derived from their goal responses, thus establishing a compound habit family hierarchy. Problem solving is conceived as a process of selection of individual habit family hierarchies, as well as the selection of specific response sequences within each hierarchy. If one particular habit sequence is tried and fails to achieve solution, the process of extinction reduces its effective reaction potential, and also the reaction potential of the entire habit family. On the other hand, previous reinforcement of a single habit sequence tends to raise the effective reaction potential of the entire habit family. Another way of raising the potential of a particular habit family in a compound is through the arousal of the anticipatory response of the habit family, which can be done, for example, by means of verbal instructions. Maltzman relates his theory to the studies of Maier, Duncker, and others, and indicates its compatibility with their general findings. He considers all forms of thinking to involve mediated generalization, and hence the compounding of previously isolated habit segments.

An experiment bearing on this theoretical formulation was carried out by Maltzman *et al.* (64), using anagrams possessing multiple solutions. The purpose of the experiment was to determine whether task instructions would induce a given set when prior instructions and training had induced a different set. Instructions for the test task were found to increase the number of "set" solutions, whereas instructions provided during a previous training task did not have this effect. In terms of theory, sets induced by instructions are conceived to be temporarily effective in producing changes in the potential of a class of responses by arousing the anticipatory goal response associated with them. In contrast, sets induced by training produce more permanent changes in the compound habit-family hierarchy.

Comparisons of individuals and groups.—Considerable interest has centered on the question of the degree to which problem solving is facilitated by group participation. Moore & Anderson (69) compared the performance of individuals and three-man groups in solving problems derived from propositional calculus [cf. (68)]. Group performance was compared to individual performance on the same set of 10 problems. The results show no significant differences between individuals and groups with respect to number of solutions, time for solution, number of steps taken, or errors.

Lorge *et al.* (59) studied the solution of a practical military problem by five-man teams and by individuals, when the stimulus situation was presented at four different "levels of reality." The problem was the "Mined Road Problem" originally developed by the Office of Strategic Services (71). This problem requires the formulation of a plan for getting a group of men across a road containing mines which cannot be neutralized or dug out. For different groups and individuals, this problem was presented by (a) verbal description; (b) photographs; (c) a miniature scale model (manipulation not permitted); and (d) a miniature scale model (manipulation permitted). The subjects in all cases were permitted to ask additional questions, which were answered when the information could be obtained in the actual mined-road field setting. Group performance of this problem was found to be superior to individual performance. No significant differences were found to be associated with the mode of problem presentation. The authors suggest that the absence of differences here may be attributed to the fact that individuals were permitted to obtain equal amounts of information about the problem, whatever its mode of presentation. Thus the importance of searching the stimulus situation is emphasized by these results.

In a second study (60) these same investigators compared the performance of individuals and teams in a field setting of the mined-road problem, under three different conditions: (a) the Actual Real Without Manipulation; (b) the Actual Real With Manipulation; and (c) the Actual Real Solve (permitting the solution to be carried to completion). Again team performances were found superior to those of individuals in every mode of presentation. No significant differences between modes were obtained, however. Although differences in mode of attack appeared between the laboratory solutions previously studied and the field solutions, differences in performance efficiency were not found. In an additional study of the data resulting from these experiments, a content analysis of written reports of solutions made by teams and individuals was carried out by Lorge *et al.* (61). It was found that teams tend to write reports that are scored inferior to their actual performance on some steps, superior on others. A similar result was obtained for individuals, but for different component steps. The written solution was in many respects not an accurate account of how teams and individuals actually solved the problem.

An analytical study of the use of "brainstorming" in the production of ideas and the invention of solutions by groups was conducted by Taylor, Berry & Block (90). This study employed three verbally-stated problems which allowed for a variety of possible solutions and considerable scope of creative approaches to solution. Performances of four-man groups were compared with those of individuals whose scores were randomly assigned to "nominal" groups. The results showed, first of all, that the mean number of ideas produced by the groups was considerably smaller than the mean number produced by individuals, contradicting previous reports about "brainstorming." Performance was further analyzed by independent ratings of such fac-

tors as feasibility, effectiveness, and generality of solutions. The performances of groups were distinctly inferior to those of individuals when compared on these dimensions. The implication is that group participation when using "brainstorming" inhibits creative thinking. The authors suggest the importance of two factors in producing this result: (a) the comparative absence of criticism in the "brainstorming" situation, and (b) the tendency of members of a group to pursue the same approach to a problem.

A summary and review of experimental studies of group problem solving is provided by Kelley & Thibaut (54) in an article which contains an extensive bibliography. These authors deal with studies comparing individual and group problem solving, factors affecting group solutions, and trends in research in this entire area.

Trouble shooting.—The armed services have encountered problems in selecting and training personnel to perform practical problem solving in diagnosing malfunctions in complex equipment. The activities carried out by an equipment mechanic in finding malfunctioning components or parts of equipment are generally called trouble shooting. On many occasions this may be mostly a matter of habitual following of a procedure. There are, however, frequent instances which cannot be tackled in this way, and which appear to involve a kind of problem-solving behavior of legitimate interest to us in this review.

Some studies of trouble shooting have as their major emphasis the prediction of individual differences in terms of correlated aptitude and personality variables [Demaree *et al.* (18); Crowder, Morrison & Demaree (15); Glaser, Hahn & Phillips (32); Grings *et al.* (39); Saupe (78)]. Often associated with these is the problem of defining the behavior required in problem solving tasks of trouble shooting in order to identify criteria [Bryan *et al.* (8); Glaser & Phillips (34); Highland, Newman & Waller (44); Fattu, Mech & Kapos (26)]. Again, attention to the criterion problem has led to the development of several varieties of performance tests which provide job-samples of trouble shooting activities, usually with emphasis on their problem solving features [Bryan *et al.* (7); Cornell *et al.* (12); Glaser & Phillips (33); French (28, 30); Rulon *et al.* (75)].

Other investigations, fewer in number, have studied the question of training requirements for effective trouble shooting, either by analyzing errors [Damrin & Saupe (16); Glaser & Phillips (34); Highland, Newman & Waller (44); Saupe (78)] or by measuring transfer of training from some specifically learned content to trouble shooting performance as measured on actual equipment or job sample tests [Crowder (14); Cantor & Brown (10); Berkshire (4); French, Crowder & Tucker (29); Moore, Saltz & Hoehn (67); Stolurow, Hodgson & Silva (86)]. Detambel (19) studied the effect of experience involving different amounts of work and different probabilities of success on the development of strategies in a task of "finding the trouble" in decks of cards. Subjects were found to adopt a strategy which conformed to probability of success, rather than to amount

of work, in this situation. Goldbeck *et al.* (35) found that subjects were able to benefit from instruction in a "half-split" strategy, in complex trouble shooting problems on a simulated electronic system.

To summarize, trouble shooting of complex equipment typically consists of problem solving which is sequential in nature; there is a sequence of hypotheses that must be tested in order to narrow progressively the area in which the malfunction is located. This characteristic is pointed out by Glaser & Phillips (33), Miller, Folley & Smith (66), and in diagrammatic form by Harlow (41). Gagné (31) has pointed out the formal similarities between equipment trouble shooting and aerial photo-interpretation. The resemblance of this problem-solving task to that of medical diagnosis and to the game of Twenty Questions [Taylor (87)] is also apparent. Each successive stage of problem solving is terminated with a checking response, which leads to acceptance or rejection of alternative hypotheses. This response typically produces an indication on a test instrument, which thus sets the stage for the next step in the progressively narrowing diagnosis.

As seen in this form the process of problem solving requires the utilization of concepts which become the terms of the hypotheses being checked ("the difficulty is either in equipment chain A or equipment chain B"). The strategies which guide the behavior from one choice point to the next appear to be relatively simple, like the "half-split" technique described by Miller, Folley & Smith (65). However, the fact that on any given occasion the final identification response (i.e., the correct solution) may be one of thousands of possible ones means that the typical trouble-shooting problem has considerable complexity. And, as Harlow (41) indicates, there are a number of ways in which the process can go wrong, including nonavailability of the correct hypothesis, irrelevant hypotheses, and use of inefficient strategies.

Comment.—In this area of central importance to thinking, new approaches and techniques would be most valuable. Saugstad's studies (76, 77) suggest that problems permitting only a single solution may be "problems" only in the sense that inadequate information is provided to the subject. The materials developed by Moore & Anderson (68) may hold promise in so far as they permit different choices to be made on the road to solution, but they need more evaluation in this respect. Maltzman's theoretical formulation (63) provides an important link to Hullian theory, although few ways of testing it have been suggested so far. The studies comparing individuals and groups in problems solving have yielded some interesting and useful findings. Generally speaking, this area seems to require most of all the imaginative invention of problems and materials which can be rationally defended as realistic representations of thinking.

DECISION MAKING

Review of the topic.—The major comprehensive review of decision-making studies and theory is by Edwards (21). This paper gives the origin and background of views about man's economic behavior in choosing objects of value, theories of decision making under conditions of risk stem-

ming from the work of von Neumann & Morgenstern (93), and the framework of modern studies by psychologists and others. One of the major roots of interest in man's decision making is economic theory concerning the choice of valued states or objects, a basic principle of which is that (economic) man behaves so as to maximize utility. Complications arise when the relations between the utilities of different goods are considered (e.g., apples vs. bananas), and this has led to the concept of "indifference curves," which map the relations between the two different types of goods. A number of methodological developments on the scaling of preferences have arisen from these questions. In dealing with situations of risk, economists have extended the notion of utility maximization to the proposition that people make choices so as to maximize expected utility (the product of the utility of each possible outcome and its probability of occurrence). But psychologists and others have been quick to point out that people do not always behave this way. This has led to several lines of investigation, and several proposed revisions of the basic theory. For one thing, there has been an attempt to substitute "subjective" utility, measured by preferences, for objective utility or money value. Second, a number of studies indicate that individuals prefer some probabilities to others, not in accordance with utility considerations. Finally, there are studies on "subjective" probability, showing that individuals tend to overestimate low (objective) probabilities and underestimate high ones. Just to complicate things more, there are studies which suggest that subjective probability is not independent of utilities, but is influenced by them. The related topic of the theory of games leads one away from the specialized subject of decision making into problems of strategies and choices among strategies in experimental games, which is one framework in which the entire process of problem solving may be studied.

Behavior in probability and gambling situations.—Cohen & Hansel (11) report a number of studies of subjective probability. Observations were made in experimental situations concerning how children and adults behave in various situations involving probabilistic or uncertain events. As a whole, these studies provide a demonstration that individuals typically behave toward uncertain events under the guidance of strategies. These strategies vary with individuals, and also display trends of development with increasing age. For example, in predicting the next of two binary events, following a predetermined series, the reasons given by children for choice indicate the existence of strategies favoring change (three heads in a row tend to be followed by a tail); luck (Joan has had all the luck and so will win again); determinate (the coin or throw is biased and therefore determined); series (heads will turn up next because it has for five times); and magic (the fifth toss always comes up heads, for some strange reason). The idea of independence occurs with increasing frequency in the older children, being reported with a frequency of 24 per cent in children aged 14 to 15.

Goodnow & Postman (37) investigated the question of how individuals make choices between two alternatives when they are guided by sets for lawful principles rather than by guesses or convictions concerning the na-

ture of chance distributions. Subjects were required to choose between pairs of cards containing two figures which differed from each other and from a key set of figures with respect to having a line from the second figure added to or subtracted from the first. Actually, the correct choice was predetermined in such a way that one or the other variation was called correct randomly and in various proportions of trials. The results showed a significantly close correspondence between the distribution of choices made by the subjects and the probabilities of the alternative outcomes. Reports of subjects obtained throughout the experiment indicated that they held a wide variety of different hypotheses, and changed them frequently. These hypotheses appeared to influence particular choices or short sequences of choices, but did not determine the general form of the distribution of choices.

Another study of the conditions determining strategies in dealing with probabilistic events was made by Goodnow (36), using a simple slot-machine with chips worth one cent each. The subjects were led to believe that the operation of the machine was automatic, whereas in fact the experimenter controlled the pay-off in random schedules of probabilities. In one group, each subject was told the experiment was one in learning in which he should look for a principle; in the other group the subject was told that the experiment concerned gambling and that he would be paid for the difference between the number of wins and losses. The results are analyzed to reveal the effects of these "problem-solving" and "gambling" strategies on the distribution of choices. The "gambling" set produced choices which approached 100:0 (the distribution of greatest pay-off in gambling) more closely than did the "problem-solving" set. Goodnow describes a number of factors which may determine the choice of strategies in this type of situation. There appear to be many reasons why individuals may show differential responses to risk, and may not necessarily follow a simple model.

The manner in which strategies based upon past experience in a choice situation can influence pattern of choices was further investigated by Goodnow & Pettigrew (38). These authors used the same slot-machine as did Goodnow. One group of subjects was given an initial set of trials with the probabilities of pay-off for the two keys set at 100:0. Another group was not given this initial experience. Without a break in the procedure, different subgroups were given 70 trials with 50:50 probabilities occurring either in "short run" or "long run" sequences. Then all were tested again in a series of 100:0 probability trials. The general nature of the hypothesis being tested was that experience with events of the 100:0 sort is conducive to the adoption of a strategy of staying with a winner and switching from a loser. The results confirm this hypothesis. Evidence is also presented which relates differing initial experiences to the adoption of a strategy of "sticking to a winner," and to the strategy of persisting with an unsuccessful choice. In sum, individuals acquire different strategies by experience with events having different probabilities; in a new situation, a previously acquired strategy may be more or less successful.

Edwards (24) carried out two experiments to investigate decisions in a two-choice slot-machine which delivered chips according to a predetermined probability sequence. The experiments were designed to discover how subjects would react to different probabilities of reward, different conditions of reward information, and different amounts of reward. Edwards draws three generalizations from his results, somewhat as follows. First, subjects tend to choose the alternative which is more frequently rewarded, but a larger reward on the less frequent side can overcome this effect. Second, the effects of variation in probability of pay-off and in amount of reward are different on the individual's asymptotic probability of choice. The greater the reward, the more an individual tends to approach 0 or 1 in his probability of choice. Finally, the evidence suggests that individuals develop two kinds of hypotheses (strategies) about reward sequences. Big hypotheses define classes of small hypotheses. Thus the individual may choose a big hypothesis such as "the left button pays off far more often than the right one." Consistent with this, he then uses a small hypothesis to guide his choice on the next two or more trials. An example might be, "the right button never pays off oftener than once every three times, so one should shift to the left button immediately after it does." Since no small hypothesis can be correct, regardless of how intricate, individuals change their small hypotheses at frequent intervals. The implication here is that people react to a truly random sequence by constantly inventing new strategies.

Irwin, Smith & Mayfield (48) studied the relationship between judgments and subject's ratings of confidence in these judgments, using packs of 500 cards containing numbers whose mean value was -2.0 , -1.5 , -0.5 , $+0.5$, $+1.5$, or $+2.0$, and whose standard deviation was 2.0 in one condition, 7.5 in another. The subjects were required to estimate, after the showing of each card, whether the average of the whole set was greater or less than zero, and the degree of confidence they felt in each judgment on a scale from -100 to $+100$. In a second experiment, subjects were asked to judge which of two packs had the higher mean, when the packs were constructed as before, and cards were displayed by pairs from the packs being compared. Confidence ratings increased with increasing numbers of cards shown and with increasing mean differences of the samples.

Using the same kind of materials, Irwin & Smith (49) conducted an experiment in which subjects were asked to tell the experimenter when to stop exposing cards in order for them to reach the decision of greater than or less than zero. The number of cards required for these decisions was found to vary directly with the absolute value of the mean. It was also greater for the higher standard deviation. With a similar technique, Irwin & Smith (50) studied the relationships between value and cost of decision, on the one hand, and amount of information demanded by the subject and his confidence in the decision, on the other. The prizes compared in this experiment were \$.50 and \$1.00; the costs per card one-half cent and one cent. Subjects were asked to rate their confidence in the correctness of their decisions on a scale from -100 to $+100$. The results show that more cards were used for

the larger prize, and also for the smaller cost. For the confidence ratings, no significant difference resulted from variation in prize, but confidence was higher with the smaller cost.

Decision-making models and related studies.—Luce & Raiffa (62) present a discussion of human decision making as a prologue to their treatment of games and game theory. The axioms of utility theory, as outlined by Luce and Raiffa, include the fundamental distinction of decision making under certainty, where each alternative course of action is known to lead invariably to a specific outcome; and decision making under risk, where each course of action leads to one of a set of possible outcomes, each outcome occurring with a known probability. In addition, the assumptions of utility theory consider the interesting case in which an individual chooses between alternatives A, B, and C (preferred in that order) when he knows he may either (a) obtain B for certain (the "certain option") or (b) take a gamble with A or C as the outcome, where he knows the probability of occurrence of A vs. C (the "lottery option"). Luce & Raiffa propose a probabilistic theory of utility intended to take into account the criticisms that individual preferences are not stable and individuals do not react to probabilities of outcomes in accordance with objective probabilities. The essential feature of the proposed model is the introduction of the appropriate mathematical expressions for probable preferences and probable expectations of outcome, in place of expressions for exact utilities and precise probabilities of outcome.

Davidson, Suppes & Siegel (17) describe another model of decision making, which proposes the simultaneous measurement of subjective probability and utility in a one-person game. The subject is asked to choose between two options, each of which is a two-element probability combination. When a whole series of such options have been selected, the model makes possible a solution for indifference between options, so that alternatives are spaced in utility in a determinate way. It thus provides a means of testing the hypothesis that, in conditions involving risky alternatives, individuals make choices so as to maximize subjectively expected utility. An experiment is reported designed to test specific derivatives of this hypothesis predicted by the model, using dice and small amounts of money. Fifteen of the 19 subjects were found to behave in this decision-making situation as if they were attempting to maximize expected utility, even though their choices were not always in accord with actuarial values. Twelve of these subjects exhibited utility curves, derived from the data, which were not linear in money. A second experiment compared the application of (a) a linear programming model and (b) an ordinal model in solving sets of inequalities of pairs of choices to obtain predicted utility functions for a set of events. In this study, music students were asked to choose between two options provided by pairings of phonograph records of classical works. As a means of predicting ordered preferences, the linear programming model was found to be superior to the ordinal model.

Hurst & Siegel (47) report an experiment designed to test the prediction of decisions from a higher ordered metric scale of utility (81). The major hypothesis tested was to the effect that individuals would make decisions so as to maximize expected utility, rather than expected objective value. A secondary hypothesis stated that latency of times of choices not predicted by the principle of maximizing expected utility would be very long. Thirty subjects in a penitentiary were employed, and cigarettes were the valued objects, with winning or losing determined by dice rolls. The hypotheses were confirmed. The authors conclude that for these subjects, utility is not linear in cigarettes (the social equivalent of money for these prisoners); their decisions can, however, be predicted by means of higher ordered metric functions.

Another approach is taken by Edwards [(23), reported in preliminary form (22)] in an experiment designed to test a model for the prediction of choices among bets. Edwards proposes a comprehensive model asserting that people choose among bets so as to maximize subjectively expected utility. The experimenter reported measured utility in five subjects by determining the number of "little bets" each subject would choose equally as often as a large bet, the utility of which is to be determined. Subjective probabilities were determined by asking the subjects to choose between a certain gain and a bet having a particular probability. After independent measures were made of utility and subjective probability, the model itself was tested with the use of a pinball machine, by making paired comparisons of various sets of bets. The model is shown to make predictions far better than chance; and to succeed better with positive expected values than with negative ones (i.e., losses).

A collection of papers of theoretical interest to this field appears in a book edited by Thrall, Coombs & Davis (91).

Comment.—A number of studies of human decision making show that individual behavior is capable of reflecting environmental probabilities. There are strong reasons to believe, however, that even when the individual is behaving probabilistically (as a "grand" strategy) he typically responds to random events by adopting a sequence of "small" strategies, one after another. Greater specificity of the variables affecting this behavior could perhaps be achieved by devising problems in which the events to be chosen do in fact fit such strategies, rather than being random. When values (such as money) are added to the situation, the behavior appears to follow laws that are considerably more complex; utility considerations affect the individual's judgments. Concerning mathematical models, it appears that they tend to do two things: (a) utilize assumptions about human decision making which go far beyond what is known about such behavior; and (b) require the construction of highly specialized situations as tests of model predictions. As Estes (25) points out, most psychologists would probably prefer a different research strategy, one in which each hypothesis about decision making is subjected to some testing before proceeding with the model.

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MOTIVATION¹

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During the past year, Boring (19) in a brief overview of the then eight published volumes of the *Annual Review of Psychology* commented that there has been "only one chapter (1952) for Motivation. Why? Probably because motivation gets picked up by personality and by social, abnormal, clinical, and counseling psychology. The Editors had better check on this point, though." I cannot speak for the Editors, but a most perplexing problem a reviewer of motivation must face arises because motivation "gets picked up" elsewhere. Boring is thus right in his over-all evaluation, although in the light of a broad conception of motivation and a review of the nine prior volumes, it is physiological, comparative, learning, developmental, individual differences, and assessment which, with personality and with social, include motivation rather than abnormal, clinical, and counseling. Motivation is thus a somewhat more popular "pick-up" than Boring thought.

What is the literature of motivation and where is it to be found? These questions face anyone starting to survey this area, and the guidelines available are scarce. No general textbook has appeared since 1936 (156). Motivation is not one of the categories used by *Psychological Abstracts*. The five volumes of the *Nebraska Symposium on Motivation* (76) are a potential guide but are not designed to serve the needs of taxonomy.

The absence of a standard guide to motivation is a problem, but "everyone" has some idea to what the term refers. A general or broad conception of motivation can be used, although such a conception may lead to inclusion of too much for some [e.g., Brown (22), Farber (42, 43)] or too little for others. My first act was to review the previously published volumes of the *Annual Review* series. I found immediately that the subjects I think of as broadly motivational or as to some degree relevant to motivation are not ignored in this series. Although Mowrer's (110) chapter is the only one to deal specifically with motivation, every volume contains a number of chapters which offer some material on motivation. Mowrer provided a good account of the historical development of motivation theory, an analysis of punishment, and some comments on appetitive drives, secondary reinforcement, latent learning, teleology and feedback mechanisms, anxiety, and ego psychology. In addition, almost every other motivational topic is covered one or more times over the nine volumes and sometimes more than once in the same volume.

The fact that the *Annual Review* volumes include many topics relevant to a broad conception of motivation is not a clear indication of what the primary literature itself shows. There must be and is selectivity in the ma-

¹ This review covers the period between January, 1957 and April, 1958.

terial used by an *Annual Review* author. The next step in the preparation of this chapter was to examine the journal literature, again with a highly general conception of motivation. This examination covered a period of 16 months, and included complete coverage of the following journals: all those published by the American Psychological Association and by the Journal Press, the *American Journal of Psychology*, the *British Journal of Psychology*, the *Canadian Journal of Psychology*, *Child Development*, *Journal of Clinical Psychology*, *Journal of Personality*, *Perceptual and Motor Skills*, *Psychological Record*, *Psychological Reports*, and the *Quarterly Journal of Experimental Psychology*, a total of 25 journals. The total number of relevant papers found approximated 450.

The journals indicated above are those in which an American psychologist would "naturally" look, as they are the ones, by and large, which are "standard." Because motivation is such a pervasive subject it seemed wise to look further. Accordingly, an accidental sampling was made of journals in other fields, totaling 39 journals. Six of these were biological-zoological journals, four physiological, six sociological-social science, four general scientific, five psychiatric, five educational, three German and French, and six were journals scattered among other fields. Although in many instances only one or a few issues rather than the complete file for the period were covered, it seems safe to say that these journals yield very little of direct interest to the systematic and experimental study of motivation. Psychoanalytic journals, however, were not represented in this sample, and certain journals devoted mainly to animal behavior in the ethological tradition were not included, because Verplanck's (149) review of last year seemed to make their inclusion unnecessary. Personnel and management journals were not examined either.

It is of interest to observe that the foreign psychological journals do not seem as much concerned with motivation as are the journals published in the United States. The issues of the French and German psychological journals examined contained little material relevant to motivation, and the two British journals examined show an amount of material relevant to motivation below the average for the United States; the Canadian journal approximates the "average" for a U. S. journal. There is a suspicion of a gradient here, but the paucity of the data on which it is predicated precludes yielding to the temptation to make further comment. One might observe, in passing, however, that motivation is perhaps the functionalist concept *par excellence*.

The journals in which the greatest quantity of motivational papers is published are as follows: *Journal of Comparative & Physiological Psychology*, *Journal of Abnormal & Social Psychology*, *Journal of Experimental Psychology*, and *Psychological Reports*; these account for over half of the 450 titles mentioned above.

I have made a parallel analysis of the various chapters in the *Annual*

Review series. The greatest amounts of materials relevant to motivation are found in the chapters on learning, comparative, and physiological, with the personality-social chapters close behind. These chapters obviously correspond fairly well to the content of the four journals already listed, which contain over half of the motivation papers.

In order to make the foregoing remarks more concrete as to the meaning of motivation, I have prepared Table I. This table displays in the first column the topics into which my notes on about 410 papers seemed to sort most easily. The percentages in the second column show the proportion of these papers sorted into each category; these percentages undoubtedly have little absolute reliability or validity but their relative sizes give a reasonable estimate of the kinds of work currently being carried out. It should be noted that certain topics do not appear which could be expected. Outstanding among these are interest, morale, group productivity, and traits or mechanisms like authoritarianism, rigidity, projection, displacement. These topics seem so confounded with variables other than motivational ones that I did not include them.

The third column of Table I shows what chapters in the *Annual Review* to look at if you are interested in one of the 27 topics. What this column means is that a given topic was treated at least once (out of up to nine opportunities) in the chapter named, so in any given volume the chapter may not yield anything important. But space and time did not permit the preparation of a volume and page index. Another thing that Column 3 of Table I shows is that almost all of the topics derived from the 1957-58 journal literature have received coverage in the review series. As a matter of fact, the only topics which one would not find in the standard chapters are "sleep" and "general."

The fourth column of Table I lists for each topic the author and volume of a pertinent paper in the *Nebraska Symposium* series (76). Many of these papers review the relevant literature, and others treat definitional, conceptual, and theoretical issues. Still others describe a research program in which the author is involved. Again the coverage of our topics is widespread. Combining the two series, the only "untouched" topic is "sleep."

Certain clusters appear among the 27 topics of Table I. A description of these clusters will make more comprehensible the kind of problem to which research on motivation is currently being directed and will also indicate the considerable degree of overlap which the categories of Table I reflect. The first eight items cluster in the sense that they represent what I perceive to be a developing reorientation in motivation, i.e., a break with the long-standing orientation toward the overriding importance of homeostatic drives and motives derived from them, with drive reduction as the critical feature of reinforcement, and with learning theory as the source of and testing ground for motivational principles. There has been a resurgence of interest in emotion (Topic 1), but this has been mainly under the aegis

TABLE I
TOPICAL CLASSIFICATION OF 1957-58 MOTIVATION LITERATURE

Topic	%	Annual Review Coverage	Nebraska Symposium Coverage*
1. Emotion, activation theory and measurement	8.0	Developmental, Physiol., Comparative, Educational	Lindley 5
2. Externally aroused behavior	7.0	Learning, Comparative	Harlow 1, Nissen 4, Seward 4
3. Study of different kinds of rewards	3.5	Comparative, Learning	Harlow 1, Young 3
4. Reinforcement without tension reduction	1.5	Comparative	Harlow 1, Nissen 4, Young 3
5. Intracranial reinforcement	1.5	Physiological	Olds 3
6. Activity	3.0	Comparative, Learning	Morgan 5, Lindley 5
7. Eating and drinking	5.0	Physiol., Comparative	Morgan 5, Lindley 5, Seward 4
8. Effects of prior experience (including sucking behavior)	3.5	Developmental, Personality, Social, Comparative, Abnormal	Nowlis 1, Sears 5, McClelland 3
9. Sex and maternal behavior	2.0	Comparative, Physiol., Abnormal	Beach 4
10. Instinct, hoarding, imprinting	2.0	Comparative	Nissen 2, Beach 4
11. Sleep	1.5		
12. Anxiety	12.0	Learning, Ind. Diffs., Personality, Physiol., Abn., Assess.	Farber 2, Brown 1
13. Avoidance, fixation and conflict	7.5	Learning, Social, Comparative	Solomon-Brush 4, Miller-Swanson 4
14. Acquired and secondary reinforcement	4.0	Learning	Brown 1
15. Intermittent reinforcement and schedules	5.0	Learning	
16. Reinforcement gradients, spread of effect, change of incentive	3.0	Learning	
17. Incentive (K) and stimulus dynamism (V)	1.5	Learning	Seward 4
18. Drive, habit formation and performance	6.0	Learning	Farber 2, Osgood 5
19. Frustration effect	1.5	Learning, Developmental, Personality, Assessment	Marx 4
20. Reactive inhibition	1.0	Learning	
21. Need for achievement, affiliation, etc.	2.5	Developmental, Ind. Diffs., Personality, Assessment	Atkinson 2, Sears 5, McClelland 3
22. Aggression and other "drives"	1.5	Developmental, Personality, Comp.	Nowlis 1
23. Motivation and social processes	3.0	Social	Festinger 2, Newcomb 1, Peak 3
24. Success-failure, aspiration, ego-involvement, selective recall	7.5	Learning, Developmental, Personality, Social, Educational	Klein 2, Rotter 3
25. Motivation and perception	2.0	Developmental, Vision, Perception, Personality, Social	Postman 1, Klein 2
26. Motivation in various groups	2.5	Individual Differences	Mowrer 1, Rodnick-Garmezy 5
27. General	2.0		Koch 4, Maalow 3, Ritchie 2, Wittenborn 5

* By author's name and volume number.

of "activation theory," which is less concerned with traditional problems of emotion than it is with the description and analysis of an energetic dimension of behavior. The study of behavior instigated and rewarded by conditions other than those involving or related to hunger, thirst, sex, and pain is another aspect of the reorientation, and it is reflected in Topics 2, 3, 4, and 5 and to some extent in Topic 6. Topics 2, 3, and 4 contain considerable overlap, since it is often difficult to decide whether a paper is postulating a kind of drive, is studying different kinds of rewards, or is contending against the tension-reduction conception of reinforcement. Topic 5, intracranial reinforcement, is distinguished by its methodology at any rate [Rosvold's chapter in the present volume reviews recent work; see also Olds (115), Zeigler (159)].

The effect of prior experience on motivation (Topic 6) seemed at one time to be mainly an offshoot of psychoanalytic emphasis on the importance of infantile experience in setting up drives and conflicts which persisted into later periods of life. As such, it would be of primary interest from the standpoint of acquired motivation or from that of the study of the effects child rearing practices [cf. Sears, Maccoby & Levin (128)], peer and other group processes, and cultural factors have in relation to the development of characteristic ways of behaving. King (77), however, has recently pointed out that the interest in early experience derives also from the critical period hypothesis and from Hebb's (63) theory of the genesis of perception and other psychological processes. The latter of these two emphases involves a number of our topics and is especially noteworthy as one aspect of the reorientation. The older emphasis on early experience continues, however, and involves other topics.

Eating and drinking and activity (Topics 7 and 8) complete the list of topics showing an altered face to motivation. As will be brought out below, recent work has stressed the role of experience in determining the patterns of response to deprivation as well as a number of physiological developments.

Topics 9, 10, and 11 represent relatively independent categories here considered together, although the role of experience in sex behavior and in hoarding has received a good deal of recent emphasis.

Topics 12 to 20, inclusive, reflect the major concerns of learning theory and the study of the learning process with motivational problems. As may be seen in Column 3 of Table I, the only chapter in previous volumes in which material on acquired drive, secondary reinforcement, intermittent reinforcement, reinforcement gradients, spread of effect, change of incentive, stimulus intensity dynamism, the nature of incentives, drive in relation to habit acquisition and performance, and reactive inhibition is to be found in the learning chapter. Anxiety (Topic 12) has a greater spread, but much of the literature in this category has arisen from the use of anxiety as a drive variable in studies of learning and performance [see Taylor

(142), and Spence (137) for review]. Avoidance, fixation, and conflict (Topic 13) also relate to learning problems, especially in so far as anxiety is postulated as a secondary drive underlying avoidance and fixation. Frustration, as a factor producing an emotional or drive state [Amsel (2)], is closely integrated with the drive features of learning theory, although the frustration category here includes other kinds of research. Kendler's chapter on learning, in the present volume, covers most of these topics.

Topics 21-25, inclusive, reflect primarily work with human subjects on such problems as the measurement and role of need achievement, need affiliation, aggression, the effects of success and failure in relation to motives, ego-involvement, anxiety, level of aspiration, and selective retention. The effects of motivation on perception and on group functioning would seem to belong in this cluster.

The rest of this review will be organized around the first cluster identified above. The other clusters have received reviews in the chapters of this series indicated in Table I, in the *Nebraska Symposium* series, and in the journal literature. Since space did not allow the coverage of everything, I have chosen to stress what seem to be the new developments.

A REORIENTATION OF MOTIVATION

Over the past eight or ten years, trends have begun to appear in the literature which portend major changes in the ways in which the motivation construct is used and the functions which it serves. Although these trends have arisen from empirical work as much as from anything, they are also reflected in general commentaries. Koch (82) has deplored the fact that motivation concepts have been largely instrumental or functional in character, i.e., that behavior is seen to serve motives which are extrinsic rather than intrinsic to it. It would appear that Nissen (112), Maslow (98), and Ryan (123) agree in this respect with Koch, as well as with various other strictures which he advances in regard to past theory and research in motivation; Koch now rejects his prior outline or "map" of the significant features of the motivational terrain (81) as inadequate, especially to the study of human problems. Woodworth (155, pp. 124, ff.) has also argued for "a behavior-primacy theory of motivation."

The empirical literature may be divided into several categories, as indicated in Table I. We will now review some of the papers which have appeared in these categories recently.

ACTIVATION THEORY

For some years Duffy (41) has argued that the intensity dimension of behavior is one major aspect of what is meant by such terms as emotion and motivation, and that a useful substitute for these words would be activation or arousal. Her first comments concerned mainly the word emotion. Lindsley (89) and Schlosberg (124) have utilized the activation

notion in their treatments of emotion, and activation theory has thus been frequently associated with emotion. Work on emotion, in this context, has shown some increase in recent years, although motivation, especially in the light of the sensory deprivation experiments [Hebb (64)] and in the light of work with the reticular formation of the brain stem [Lindsley (90)] seems on the way to integration with activation theory. Very briefly, we may summarize activation "theory" as asserting that many behavioral phenomena, ordinarily termed emotional or motivational, can best be interpreted as stemming instead from the level or degree of activation, arousal, excitation, or energy mobilization of the organism; various indices of degree of activation, independent of the behavior being investigated, have been proposed: EEG patterns, galvanic skin response, pulse, respiration, muscle tension, etc.

The recent research has mainly been concerned with the relation of some performance to level of activation; it is usually argued that an inverted *U*-shaped function will link the two measures, meaning that behavioral efficiency will be optimal at an intermediate level of activation but will be less than optimal as activation is either very low or very high. A recent experiment by Stennett (138) illustrates the problem studied and the methods used. Stennett recorded muscle potentials from the arms of his Ss (and also palmar conductance) while the Ss performed a tracking task. Several incentive or motivational conditions were employed: a relaxed or no incentive condition, a high incentive condition, and an intermediate, or optimal, condition (defined in terms of the four trials in which the best scores were made under moderate incentive pressures). The results show, for example, that the Ss in the no incentive condition made more errors than those in the optimal incentive condition but that the no-incentive group had lower scores for palmar conductance and for electromyographic data. The high incentive group also made more errors than the optimal group but, in this case, its scores for conductance and muscle activity were higher than those for the optimal group. The latter group thus occupied an intermediate position in arousal but was the best group in terms of errors. Malmo (93) has brought together a number of studies of this type, together with several in which the Ss were psychiatric patients. The patient groups, especially under stress conditions and especially those with high loadings of chronic anxiety, tend to be more reactive than controls.

Other recent experiments have related muscle tension to eye blink rate [King & Michels (78)], serial rote learning [Berry & Davis (15)], and performance on a rapid discrimination problem [Belanger (11)]. It should be noted that the tension found in different muscles does not always relate equally well to task performance or correlate well between muscles. Thus, Berry & Davis (15) report significant positive rank coefficients between muscle potentials from the forehead and from the jaw but no correlation for these potentials and those from the arm. Martin (94) reported an

insignificant relationship between forehead and forearm tension in a comparison of a rest condition and a question period. Careful attention to the muscle groups used, as well as their relation to the muscles used in various tasks, may prevent the piling up of conflicting reports on the relationships between tension and performance.

Somewhat related to these reports of an association between muscle tension (or palmar conductance) and other performances is the study by Fuster (47). Fuster stimulated by means of implanted electrodes the reticular formation of several rhesus monkeys while they were performing a discrimination. The finding was that reaction time and per cent of correct choices were improved during such stimulation as compared to control trials. Fuster suggests that the stimulation served to arouse "basic attentive behavior," thus facilitating performance. Presumably, the relation between muscle tension and performance and between palmar conductance and performance, as represented in Stennett's experiment (138) and others cited above, involves processes similar to those more obviously directly involved in Fuster's study—when the reticular activating system is optimally aroused, behavioral efficiency is incremented. This is at least what activation theory would seem to hold, although it is not essential to the general viewpoint that the reticular formation be involved.

A good deal of work has appeared this year involving the galvanic skin response (GSR). Much of this is related to problems not of direct concern to activation theory, but Burch & Greiner (23) examined the relationship of GSR to pentobarbital, metrazol, and prolonged wakefulness, and found two types of GSR fatigue patterns. In using GSR and other measures as indicators of arousal, it is probably wise to keep in mind the point emphasized again this year by Lacey & Lacey (83), that stressors produce idiosyncratic patterns of autonomic response in different Ss but that these patterns, for a given S, are relatively stable from stressor to stressor. Block (16) has reported personality differences in Ss who show high and low GSR reactivity in a lie detection situation.

Emotionality as a trait of an organism is probably related to degree of chronic activation or to responsiveness to transitory stressors. Mahut (92) has shown that the effects of presentation of emotional stimuli to dogs vary as a function of breed. Thus boxers and terriers were "fearless" as compared to, for example, poodles, dachshunds, German shepherds. However, dogs reared in a restricted environment differed in emotional reaction from dogs of the same breed reared normally. An interesting finding is that the restricted environment tended to reduce the emotional response differences in the breeds subjected to it. Broadhurst (21), who studied rats with the open field test, found sex differences and strain differences in emotionality as measured by defecation and by ambulation scores. Together with prior research in this field, these studies probably indicate genetic

differences and in any case point to a factor which needs to be taken into account in comparing studies of emotionality in relationship to other behaviors.

As a final entry in this section, we may note Broadhurst's demonstration (20) that the Yerkes-Dodson law holds for rats in a discrimination task. Broadhurst also reports the relationship between underwater swimming speed in a straightaway and duration of air deprivation prior to the swim. Speed rises in a negatively accelerated fashion as air deprivation increases to 10 sec. and then is essentially stable for the other durations used (up to 25 sec.). Also reported is the fact that the more emotional animals learned better than the less emotional animals at minimal motivational levels, but this difference disappeared when the drive was at higher levels.

EXTERNALLY AROUSED BEHAVIOR

Perhaps even more significant in research output than activation theory and other aspects of emotion is the recent re-emphasis on external factors which may arouse behavior. Included here are manipulation, exploration, and curiosity motivations, as well as the indication arising from sensory deprivation experiments [Hebb (64, 65)] that organisms are disturbed by and function poorly when submitted to extreme sensory deprivation conditions for substantial periods of time. Activation theory, as Hebb (64) indicated, may be stressing essentially the concept of generalized drive which learning theorists [Spence (137)] have discussed for many years. The work relevant to this and the next two sections, however, seems directed to show that there are fairly specific but nonhomeostatic drives, that a great variety of reinforcers exists which cannot be regarded as secondary reinforcers, and that there may be learning without tension reduction or even with a moderate increase in tension or stimulation. Activation theory seems consistent with this last point, as there is presumably an optimal level of stimulation which is considerably greater than zero.

Exploratory behavior has been observed in the animal laboratory for many years [Dashiell (33), Richter (120), Nissen (111)]. The recent literature, however, seems to have sprung for the most part from other problems. Montgomery (107) was originally concerned with the exploration of alternation behavior at choice points in the maze from the standpoint of reactive inhibition, and his conception of an exploratory drive, as well as Glanzer's stimulus situation hypothesis [(52); see, also Denny & Adelman (39)], seems to have developed out of work on this problem. According to Hebb (65), the work on the effects of sensory deprivation on behavior arose from investigations designed to get at some of the factors in "brainwashing." Harlow's work (61, 62), however, appears to have arisen directly

from interest in the existence of manipulation motives, on the one hand, and on the other, from a belief that stimulations, like visual stimulation, might have directly rewarding effects.

Of the several subareas suggested in the preceding paragraph, studies of exploration have been most frequent in the past 16 months, and several aspects of the problem have received attention. Carr & Williams (28) observed exploratory Y-maze behavior in purebred albino, black, and hooded rats, and found the hooded animals to engage in significantly more exploratory behavior than did the other strains. One of the two strains of hooded rats that Broadhurst (21) used in his study of genetic factors in emotionality was among his most ambulatory Ss. Unfortunately, further comparison of these two studies is not possible at present.

One of the major problems in relation to exploration is to define the characteristics of stimuli which are associated with exploration. I have a rather strong impression that these will turn out to be very similar to the classical lists of factors involved in "attention." Dember & Earl (36) have suggested that exploration, manipulation, and curiosity belong to the "attention" class of behaviors and have argued that attention is aroused by stimulation which departs from the subject's expectation. Stimulus complexity and temporal change are two ways in which stimuli may depart from expectation, but Dember and Earl suggest that the "complexity" of the organism itself will be important to the effects of such stimulus variables.

Berlyne (12, 13) has reported work which suggests that the stimulus variables which determine attention (orienting responses) are similar to those which control curiosity. Attention was here defined as the duration of S's fixation upon a stimulus picture, whereas curiosity is defined by the number of times S will expose himself to a picture when each exposure is a brief one. Fixation times were greater for more complex figures than for less complex figures; furthermore, when two pictures were always presented side by side and when over a series of trials the picture on one side was always the same and on the other side was always different, S spent more and more fixation time on the side where the picture varied. Since fixation is an orienting response (attention), complexity and variation of stimuli are thus shown to control attention. When S could express "curiosity" through exposing pictures by pressing a key, it was found that more responses were made for pictures exhibiting incongruity, unexpected changes (in a series), and entropy than for less "interesting" pictures. More detailed experimentation will be needed to specify the exact relationships among attention, perceptual curiosity, and Berlyne's notion of "epistemic" curiosity, but I would guess that the stimulus variables controlling all these processes are quite similar.

Berlyne & Slater (14) have attempted to separate the factors in exploratory behavior of novelty, complexity, and spaciousness in the goal box,

variables which are often confounded in experiments on exploration. In a Y-maze the arms of which terminated in either a blind alley or a "spacious" goal box, rats developed a preference for the turn leading to the latter. In another experiment, one arm was empty and the other contained a variety of stimuli, which, however, were invisible from the choice point. The animals developed a preference for the turn leading to the stimulus variety and latencies for response to this side were also significantly shorter than those for the other side. These experiments show the effects, respectively, of spaciousness and complexity of objects in exploratory behavior. In a third experiment, novelty was studied. Cards bearing designs could be inserted in the arms of the maze as well as in the start box; in one arm the same card as that in the start box was present on every trial, while in the other arm the card found was different from the one in the start box and also varied from trial to trial. Neither alley choice nor latency measures yielded a difference indicative of the development of an alley-preference, although the Ss spent more time sniffing the novel cards than the familiar ones. Dember, Earl & Paradise (37) have also studied the effect of stimulus complexity. They determined whether animals would spend disproportionate amounts of time in either loop of a figure-eight kind of apparatus, when the two loops differed in the complexity of the visual patterns they presented. The results showed that, as trials progressed, the rats spent more of their time in the more complex loop, a finding which the writers consider inconsistent with a stimulus satiation explanation of exploratory behavior.

Welker (153) has studied the effect on exploration of requiring S to remain in an enclosure as compared to giving him a choice of entering it. Operationally, this difference reflects the procedure of placing the rat directly into an enclosure versus permitting him to enter it from a small carrying cage. During the forced trials Ss were more active than during the free choice trials, but this exploratory behavior declined over a 5 min. observation period in the forced procedure but did not decline in the choice procedure. Between forced sessions, there was a recovery of activity in the first minute of the period, but this occurred to a smaller degree between free sessions. However, over a series of free sessions exploratory activity increased, but it did not increase over a series of forced sessions. Essentially similar results were found when the enclosure was equipped with "objects" as when it was empty, although the presence of the objects reduced the differences between the free and forced procedures. Welker points out that his results suggest the possible presence of avoidance in his forced procedure, although perhaps the safest conclusion from this study is that exploratory behavior is significantly affected by apparently minor variations in procedure.

The foregoing experiments, as well as the earlier ones in the literature, have stressed visual stimulation in exploration. Glickman (53), however,

has reported that hooded rats whose eyes were enucleated showed more exploration in both an open-field test and in a Y-maze than sighted rats and that the blinded animals showed more exploration in the second of the procedures they experienced than in the first, whereas sighted Ss were not so facilitated. Glickman interprets his findings as indicating that blind animals, in order to maintain an optimal level of stimulation, must explore more than seeing rats, because of the lack of visual stimulation. The evidence Glickman reports is in line with this supposition, which he would also advance as a general proposition that exploratory behavior serves to maintain an optimal stimulation level rather than to express an exploratory drive. Other explanations are no doubt possible.

The experiments with rats described so far have usually used extent of locomotion in a maze or in some other enclosure to assess exploratory behavior. Another area of investigation, stemming more directly from concern with factors responsible for alternation in choice behavior (e.g., reactive inhibition), has used preference for alleys as the critical index. Denny (40) has studied stimulus satiation effects in an experiment designed to test his theory (39). His animals were run in a T-maze, one arm of which was painted and the other natural wood. Blocks of six trials were given over three days, two spaced trials on each day. All trials were terminated with food reinforcement. In each block, free and forced trials were arranged so that four of the six trials were to the painted side (or to the other side). There were eight repetitions of the blocks and there were additional free trials thereafter. The question was whether, on the free trials, the rats would develop a preference for the less frequently visited side, and the results showed a progressive trend in this direction. Denny believes a stimulus satiation hypothesis can account for these findings but that the spacing of trials which his experimental design permitted makes an interpretation based on reactive inhibition untenable. Sutherland (141) studied a somewhat similar problem by arranging two mazes so that they were identical up to and beyond the choice point but the two paths in one of them led to different and distinctive goal boxes, whereas the two paths in the other maze led to the same goal box. The subjects were rewarded on all trials. In the maze with two goal boxes trial to trial alternation of pathways was significantly more frequent than in the maze with a single goal box, suggesting an avoidance of the goal box last visited. Similarly, in a second experiment, it was observed that animals preferred in one goal box tended, when placed at the start box, to run on the path which led to the other goal box.

These experiments, together with others in the exploration literature, suggest that avoidance of a recently visited spot is a significant feature of the behavior of the rat. A negative note, however, is sounded by the report of Levine, Staats & Frommer (88), who failed, in several experiments, to

obtain a preference for the changed arm of a Y-maze, although prior experiments had done so. It is not clear what factors may have been responsible for these negative experimental results. Williams & Kuchta (154) also failed, in one of their experiments, to obtain much exploration of a dissimilar Y-maze section, made dissimilar when one of the three black arms of the maze was changed to a white one. In a second experiment, they obtained an increasing amount of exploration over a series of trials in an arm which contained "objects."

It seems to be well demonstrated that rats will explore when apparently unmotivated by hunger, thirst, or sex drives, and that they tend to avoid a recently visited location. These two statements of fact may well amount to the same thing, although they derive from somewhat different experimental operations. The mechanisms of exploratory behavior remain to be convincingly demonstrated, although earlier work has already rejected the role of the primary drives mentioned above and of general activity in powering exploration; fear has been found to inhibit exploration. Several experiments in the time span under review have been concerned with the evaluation of possible explanations. Lubow & Tighe (91) tested a stimulus discrepancy hypothesis of motivation by measuring activity in a stabilimeter kind of compartment. In one group adaptation periods were followed by varying amounts of increases or decreases in illumination. Activity was found to increase as a positively accelerated function of the logarithm of light-intensity change but did not change when the intensity was decreased. While this experiment did not study exploration as such, an hypothesis relating stimulus discrepancy to exploration is a legitimate one; this study confirms the hypothesis only for the increase in intensity.

Montgomery & Zimbardo (108) tested the hypothesis that an exercise or a boredom drive might underlie exploration. They also believe their data to constitute a reasonable test of another explanation of exploratory behavior, that, in the history of an animal, activity is rewarded and hence learned because being active is associated with obtaining food and water. These hypotheses were tested by rearing different groups of animals in either a normal laboratory cage, a small cage, or a small cage together with sensory deprivation. At 21 days of age (weaning) the rats were placed in these conditions and maintained there with *ad lib.* food and water for 25, 50, or 100 days, at which point they were tested for exploration in a Y-maze. Exploratory behavior showed in neither amount nor orderliness any relationship to type or duration of rearing condition (or to sex), a finding which the authors hold to refute the exercise and boredom drive hypotheses and also the hypothesis that exploratory behavior is learned through reinforcement. Their results conflict with the ones reported for dogs earlier by Thompson & Heron (145), who found that restriction of activity earlier in life led to increased activity in exploratory situations at a later time. However, Thomp-

son & Heron's results would seem no more consistent with a learning interpretation (based on reward) of activity or exploration than those of Montgomery & Zimbardo, although boredom or exercise drives cannot be ruled out.

Another experiment by Zimbardo & Montgomery (161) compared rats reared in normal cages with rats reared in "free-environment" cages. The latter contained objects and no doubt constituted a "stimulating" environment. The animals lived in their respective cages for 25, 50, or 150 days following weaning and were tested on the Y-maze apparatus. There were no differences between the 25-, 50-, or 100-day groups in exploration, but the female rats in the normal cage groups explored significantly more than any other subgroup. Another finding in which the results of this experiment differ from prior ones is that exploration declined over the four days of testing instead of recovering between days, as other data had shown. It is not possible, on present information, to evaluate the odd findings of this experiment. Charlesworth & Thompson (30), however, conducted a less radical version of the restricted-environment experiment; their experimental rat subjects lived in individual confinement boxes for either 3, 6, or 9 days, and half the Ss in each of these groups lived in the dark, half in light. Control Ss lived alone in ordinary cages for 10 days. The groups did not differ significantly in amount of exploratory behavior during the observation periods, suggesting that a reduction in visual variety is not a necessary condition for exploration. Miles (101) showed that kittens would learn a discrimination in which reward consisted of objects in a box on one side, the box on the other side being empty. After extinction, this discrimination was successfully reversed, with escape from the box and exploration of the room being a more effective reward than commerce with a familiar food dish. However, under 22 hr. hunger, animals learned another discrimination for food reward faster than for the room exploration reward. Miles believes that neither manipulation of objects nor exploration have ever been associated with reward in these kittens, so that a secondary reward interpretation of the findings is inapplicable.

Zimbardo & Montgomery (160) have presented another test of the role of hunger and thirst in relation to exploration. Hungry and thirsty animals explore less than satiated animals, especially when food or water is available, probably because there is conflict between consummatory responses and exploratory tendencies. Zimbardo & Miller (162) used the running speed for entrance into a second compartment as the dependent variable, rather than number of units explored. They found that hunger tended to affect this measure but are unable to say whether this effect is mediated through a curiosity drive or through an effect of hunger on the performances reinforced by curiosity. In any case, they conclude that novelty is a reinforcing state of affairs. Chapman & Levy (29) have shown that novel stimuli are

reinforcing, even when the novel stimuli are present only at the termination of a behavior sequence, that food deprivation appears to decrease the reinforcement value of a novel stimulus (a point not in keeping with certain of Zimbardo & Miller's results), and that prior experience with reinforcement by a novel stimulus appears to increase the reinforcing effectiveness of further novel stimuli.

It is probable that the final word concerning the relationship of hunger and thirst to exploration has not been spoken, but different methodologies will apparently make a difference in the relationship found. Beach (10), in showing that injections of morphine increased exploration in his rats, failed to observe a depressant effect of hunger on exploration, an effect found in some earlier studies.

Only two experiments on sensory deprivation effects in human subjects appeared during the period. Vernon, McGill & Schiffman (148) showed that visual hallucinations occurred only when some light could be perceived by their Ss during the sensory deprivation experience. When light was totally excluded, hallucinations did not appear. Vernon & McGill (147) found little if any difference in efficiency of rote learning for sensory deprived Ss and controls, a finding at variance with other studies of intellectual processes under these conditions. Such experiments suggest that much information is needed to permit a full assessment of the nature and conditions of sensory deprivation effects.

KINDS OF REWARDS

The study of the attractiveness and reinforcing value of various incentives is obviously closely related to exploratory behavior. However, the experiments now to be cited reflect primarily a concern with description of reinforcers rather than concern for drives perhaps related to them. Cho & Davis (31) have shown persistent and differential responsiveness in monkeys to objects made in a variety of shapes and of several materials. Young & Asdourian (157) compared the choices of rats for sucrose solutions against a 1 per cent saline solution; all sucrose solutions were preferred, and extrapolation from the results suggest that a 0.35 per cent sucrose solution would be isohedonic to a 1 per cent salt solution. This hypothetically isohedonic solution is, however, below the threshold for sucrose. Smith & Duffy (134, 135) suggest that sugar solutions have both drive-reducing and sensory-satisfaction properties, and that the reinforcing value of saccharine is related to hunger. Garcia & Kimeldorf (48) made saccharine an aversive stimulus by associating it with gamma irradiation.

Butler (24) showed that deprivations of visual experiences up to 8 hr. in length were associated with an increased frequency of performance by monkeys of a task the reward for which was visual exploration. Butler & Harlow (26) found evidence for the learning of color discrimination for

visual exploration rewards in rhesus monkeys but did not observe the formation of learning sets (interproblem learning). They attribute this failure to nonmotivational factors. Butler (25) showed that monkeys would learn a position discrimination when silence was broken, after a lever press on one lever, by 15 sec. of noise from the monkey colony (as feeding time approached); the possibility of secondary reinforcement is hardly excluded here. Barnes & Kish (6) found that mice would learn to stay on a platform or to stay off of it if these responses were associated with the termination or prevention of the onset of an intense white noise. During a preliminary adaptation stage, the rats were found to spend most of their time on the movable platform, thus confirming Kish & Antonitis' (79) prior observation that this kind of stimulation is reinforcing. Reduction in white noise is stimulus reducing, although the movement of a platform is perhaps not. Jerome *et al.* (75) found that rate of crossing from a lighted chamber to a dark one increased as the logarithm of light intensity in the first chamber. This result also appears consistent with a stimulus reduction interpretation.

Onset of illumination, in earlier experiments, has been found to reinforce behavior. Stewart & Hurwitz (139) reinforced hungry rats for lever pressing with a dim light in an otherwise dark box under continuous reinforcement, 3:1 and 6:1 reinforcement ratios. The 3:1 ratio gave higher response rates than any other condition, whereas food and water reinforcers at the higher ratios usually yield the greater rates.

Several experiments have compared response to light reinforcement in relation to conditions of deprivation. Hurwitz & De (71) found no differences in rate of lever pressing for dim light reinforcement when the same rats were studied under 7, 17, or 22 hr. food deprivation. When different Ss were run under 6, 12, or 22 hr. food deprivation, the highest rate was found for the 6 hr. group. Several unavoidable shocks in a different apparatus just preceding lever pressing facilitated responsiveness to light reinforcement, although in Ss in which light was associated with the shocks no such facilitation appeared. Clayton (32) found that rats deprived of water for 22½ hrs. responded at a higher rate for a light reward than sated animals, and Forgays & Levin (45) found similar results when they compared 22 hr. food-deprived rats with rats that were not deprived. These results are superficially in conflict with those of Hurwitz & De (71), who, however, ran no control (satiated group). It is possible that the function relating food deprivation and response rate for light reinforcement is curvilinear with a maximum at relatively low levels of food deprivation. The results of these several studies disagree with those reported by Kling, Horowitz & Delhagen (80), who did not find water-deprived rats to be reinforced by light.

Other studies which can be mentioned include one by Weiss (152), who showed for rats that lever pressing was reinforced when it turned on a heat

lamp in a cold Skinner box; one by Mason & Harlow (99), who found that under some conditions infant monkeys could learn to approach the feeding situation; and one by Terrell & Kennedy (144), who found that candy-reward was more effective for children than either praise, reproof, or token rewards. Seidman *et al.* (129) showed that the strength of self-administered shock which their human Ss would tolerate was higher when a partner apparently shared it than when S was alone. Dennis (38), who used a critical incident technique, found differences in the situation of reward and punishment experienced by children of several groups in the Middle East. He suggests further study along these lines in the attempt to understand cultural differences.

REINFORCEMENT WITHOUT TENSION REDUCTION

Light onset as a reward could have been included here, rather than in the prior section. Intracranial stimulation could also be reviewed in this connection, but Rosvold's chapter in the present volume, together with the reviews by Olds (115) and by Zeigler (159), makes its inclusion unnecessary. Several studies may be reviewed, however, which were more or less directly concerned with the reinforcement process [cf. Woodworth (155, pp. 119-124)].

Zeaman & Wegner (158) have reported another study in their cardiac conditioning series; the new study involved very short (0.1 sec.) and long (15 sec.) shocks as the unconditional stimulus (UCS). Contrary to expectation, conditioning occurred in both groups, and detailed analyses of the heart response records suggest that the conditional response (CR) developed is not related to what the heart is doing at the time of shock cessation. A drive reduction interpretation on these data is no longer tenable. Porter (116) did not find that cessation of shock caused an increased repetition of the immediately preceding word-number pairs in a guessing experiment, as he expected from a drive reduction interpretation. Beach (9) has apparently demonstrated that rats will learn to prefer the goal box in a Y-maze which is associated with the post-morphine-injection euphoria. He believes this euphoria not to be a drive reduction process. Boguslavsky (18) omitted the shock UCS in a leg flexion situation with goats and found the probability of CR to a tone to be reduced by this procedure, although omission of UCS should be fear reducing, hence rewarding, and should increase CR frequency. Boguslavsky believes the omission of shock caused a prolonged maintenance of a preparatory set, followed by a period of agitation which upset the animal's ability to respond to the next tone by being vigilant in preparation for the next shock. It is not clear to me just what drive-reduction theory would predict in this situation, which is perhaps more complex from this viewpoint than Boguslavsky suggests.

Hughes (69) showed that the per cent of correct choices in a T-maze in-

creased with both volume and concentration of a saccharine solution. However, for large volumes of solution a medium concentration was superior to other concentrations, but at low volume the highest concentration was most superior. Concentration and volume of saccharine solutions are thus complexly interrelated in reinforcement; perhaps both taste stimulation and amount of responding are involved in reinforcement. Hovorka (67) trained rats to run to food which they obtained by reaching over a bar; bar depression caused shock to be delivered at either high or low intensities, producing convulsions. The question was whether avoidance, based on shock, would develop. One group had a convulsion rapidly induced on the bar press, and it persisted in bar pressing, showing less avoidance than the group which had a slowly induced convulsion on the first bar press. Hovorka, on the basis of this experiment, does not recommend electroconvulsive shock as a trial terminator in order to protect the learned response from interference, as both groups did eventually develop avoidance.

ACTIVITY

To many theorists, adaptation to the environment would appear to be fostered if animals become active when states of tissue-need develop; presumably the animal which, in a state of need (or drive), becomes active will, in moving about, increase the probability that it will encounter substances essential to the reduction of tissue needs [cf. Hull (70, pp. 17-18)]. It seems to be assumed that increased activity under need is a sort of automatic, unlearned process, and this assumption is consistent, of course, with the general notion that one can measure drive by studying activity and consummatory behavior in relation to duration of deprivation.

The recent literature on activity (and on consummatory behavior; see next section) has emphasized the role of experience in determining its relationship to deprivation, thus questioning the automatic role of drive in energizing or activating behavior.² Richter (120), who is usually quoted to the opposite effect, actually anticipated this notion, at least for the rat, when he suggested that motility rhythms in the young rat perhaps develop through trial and error learning under hunger drive and reward (120, p. 319).

That activity patterns might be learned was studied by Sheffield & Campbell (132), who associated a stimulus change with delivery of daily food

² The interpretation that activity, eating, drinking, and pain are subject to influence by experience and that they are not thus pure indicators of motivational states, is close to one made by Dalbir Bindra in his forthcoming book, *Motivation: A Systematic Reinterpretation*. I believe I had reached this interpretation independently, although I read the first five chapters of Bindra's manuscript before writing this paper. In any case, it is a pleasure to acknowledge the agreement between Bindra and me.

ration in their experimental groups but not in the control group. Activity rose during the stimulus change for the experimental groups but did not in the controls. Finger, Reid & Weasner (44) and Reid & Finger (119) conclude that, if feeding immediately follows running periods in an activity wheel, activity increases with experiences of deprivation, whereas it does not do so in animals for whom feeding occurs at least an hour after running in the wheel. Seward & Pereboom (130) also noted the possibility that "spontaneous" activity is learned.

These studies have manipulated conditions in order to determine whether activity and deprivation states are related through learning or not. Closely related are studies which have charted the course of activity as animals, initially naive with respect to deprivation, experience a restricted feeding or drinking schedule. Earlier work has suggested that the activity picture changes slowly but progressively over a prolonged period on a restricted (i.e., one hour a day) feeding schedule. Reid & Finger (118) repeated an earlier experiment with certain added controls but still find that at least 15 days must be allowed for rats to develop a stable adjustment to a feeding schedule, as measured by activity. This experiment, together with the earlier one, is important not only in indicating that activity does not immediately reflect need but also in suggesting that, in learning experiments, a habituation period to restricted feeding should probably be maintained longer than has usually been the case in order that motivational levels be constant. Hall & Cannon (59) have added the point that, as measured by activity, 5 hr. of deprivation is a quite different thing if the 5 hr. follow access to food for the other 19 hr. a day, or, alternatively, if the 5 hr. are the first 5 of 23 in animals allowed access to food for only 1 hr. a day.

Another aspect of the activity problem was raised by Campbell & Sheffield (27), who, on the basis of their results, argued that food deprivation only sensitizes animals to be reactive to stimuli rather than "pushing" or "driving" them into activity. Hall & Hanford (58) and Hall (56, 57) have obtained results differing from those found by Campbell & Sheffield. However, Hall's results were obtained with activity wheels, whereas Campbell & Sheffield used a stabilimeter type of cage. Results from a wheel are difficult to interpret, since, as Woodworth (155, p. 61) suggests, wheel running includes stimuli for continued running. Strong (140), using an apparatus like Campbell & Sheffield's, found no change in activity with deprivation over 48 hours. A more sensitive stabilimeter actually showed a drop in activity over 72 hours of deprivation in another group of animals. The stabilimeter and the wheel give different results, as has long been known. It is a question which type of activity has more significance as an indicator of deprivation states. Griffiths & Griffiths (55) report activity as measured in both wheels and a checkerboard device to be reduced by reserpine, but the finding was clearest for wheel activity.

The status of activity as an automatic indicator of drive strength is certainly questioned by these studies, together with earlier ones in the fairly recent literature. [Harker (60) has reviewed over 300 references on diurnal rhythms.] Davis (34) has questioned the utility of the homeostatic concept as a general, adaptive principle, a point pertinent to the view that activity is a means which facilitates the restoration of balance to the disturbed equilibria present when tissue deficits develop. Pertinent to the process of adaptation, though not to activity, is Boguslavsky's (17) analysis of the process by which a conditioned response changes its form from that seen in unconditioned response to something else. He concludes that this change is consistent with a contiguity interpretation of learning and that therefore the contiguity principle is not inconsistent with adaptational processes.

EATING AND DRINKING

The study of these response processes proceeds in a variety of directions. As with activity, there has been recent emphasis on the role of experience in determining eating and drinking patterns. Ghent (50, 51) finds that rats, initially naive with respect to deprivation, require experience of deprivation in order to respond quickly and adequately in the eating or drinking situation when deprived of food or water. These observations are consistent with those of Reid & Finger (117), Lawrence & Mason (84, 85) and Baker (4), all of whom showed food intake to initial and continued food deprivation schedules to require considerable periods of time to stabilize and that food intake patterns are affected considerably by past experience. Baker (3) also questioned whether the rat maintained under *ad lib.* feeding conditions exhibits a feeding cycle. While he found a mean interesting period of 125 min., a value similar to that reported by Richter (120), the standard deviation was 92 min. and study of runs of feeding and no-feeding periods did not suggest rhythmicity in feeding. Bare (5) has found rate of eating to be largely determined by time of day, even under deprivation conditions. Siegel's observation (133) that college students tend to eat an entire serving rather than partial amounts probably fits with a learning factor in consummatory behavior. As with activity, these findings suggest that consummatory behavior, in the animals studied (mainly rats), is not a pure index of drive strength, uninfluenced by experience. It should also be kept in mind that different measures of consummatory behavior, as already indicated for activity, yield considerably different results for the same deprivation conditions [Miller (102)].

Miller (103, 104) and Morgan (109), as well as Lindsley (90), and Olds (115), have brought together the recent work relating central mechanisms, central stimulation, and drugs to eating and drinking. A review by Adolph (1) is also valuable. Miller (103) has summarized the experiments of his group in which oral and stomach factors in reinforcement and intake have

been compared. Miller, Sampliner & Woodrow (105) found that oral administration of water caused a greater decrement on subsequent measurements of drinking than administration of water directly into the stomach, a finding paralleling earlier ones with food. James & Gilbert (73) found that injection of food into the stomachs of puppies just past the weaning stage eliminated consummatory behavior immediately thereafter, in contrast with another observation that food injection did not eliminate sucking responsiveness in nursing puppies [James (72)]. Smith & Duffy (136) did not obtain completely parallel results to some of those reported earlier by Miller and his associates, although the methods varied a good deal. Smith & Duffy believe that it is bulk in the stomach that inhibits eating.

Several studies have examined the effects on eating or drinking of hypothalamic lesions [Teitelbaum (143)], cold [Weiss (151)], atropine [Schmidt, Moak & Van Meter (126)], pentobarbital [Schmidt (125)], chlorpromazine [Schmidt & Van Meter (127)], and radiation [Heimstra, Davis & Steele (66); Shaklee & Miller (131); Davis (35)]. Space does not permit these studies to be further described or to deal with the interesting experiments on the effects of water or NaCl preloads in the stomach [O'Kelly, Falk & Flint (113, 114)], and of subcutaneous injections of hypertonic saline [Wayner & Reimanis (150)].

PRIOR EXPERIENCE

The effect of prior experience has already been brought out in relation to activity, eating, and drinking. Melzack & Scott (100) reared Scottish terriers in restricting cages from the ages of four weeks to eight months; as compared with controls, these dogs were much retarded in learning to avoid a shock or to avoid the person who had burned their noses or pricked their skins. The writers apparently feel that not only avoidance learning was impaired in the restricted animals, but also that the perception of painful stimuli was abnormal. The suggestion is that pain is a learned reaction. In a different situation, Freedman (46) showed that rearing experience affected certain breeds of dogs (including wire haired terriers) more than others. Mogenson, McMurray & Jaques (106) found that stress and the administration of cortisone nullified the weight gains often seen in gentled rats, and Gertz (49) found little lasting effect of earlier handling on emotionality or on a water T-maze performance. Baron, Brookshire & Littman (7) found relatively little difference between rats traumatized by shock early in life and as adults in avoidance and escape training at a later adult age, although both groups differed somewhat from controls. Levine (87) found that rats not handled in infancy showed less weight gain and less water consumption in a brief drinking test after their first experience of water deprivation than rats either handled or shocked in infancy. The literature on sucking behavior has been reviewed by Ross, Fisher & King

(122). Experience (mainly isolation and different social groupings) is seen to influence sex behavior in guinea pigs [Valenstein & Goy (146); Riss & Goy (121)], and broodiness in ring doves [Lehrman (86)], but not sexual behavior in the male rat [Beach (8)]. Howard (68) interfered with sexual responsiveness to females in male rats by associating the females with electric shock. Marx and his associates (95, 96, 97) have reported several experiments which support his learning interpretation of hoarding. Jaynes (74) showed that apparent imprinting of chicks declined with age (up to 60 hours) but that at a later test the Ss imprinted at later ages showed a rising "retention" curve, whereas the chicks imprinted younger showed a decline in retention. Gray & Howard (54) found that chicks imprinted at 31 hours of age would go 24 hr. later to the human experimenter who had imprinted them, suggesting that imprinting is to an individual and is not species learning.

CONCLUSION

The major conclusion I would reach from the material reviewed here is that, if present trends continue, motivation as a distinctive concept, coordinate to other psychological concepts, may well disappear. It apparently has no automatic response indicators, if it is true that activity, consummatory responses, and reaction to pain are or may be heavily influenced by learning and experience. Vigor of response, also, is known to be influenced by learning. On the antecedent side, the rejection of homeostatic drives as sufficient to motivation also makes insufficient the deprivation operation in the control of motivation. Unless a limited number of principles can be found which underlie the various stimulus situations which arouse or reinforce behavior, one might as well, with Nissen (112), argue that every act has its own intrinsic motivation. While this may very well be true, it would seem to deprive motivational description of any distinctive value.

Activation theory may hold some promise, but its present appeal lies not so much in its postulated relationship between tension level and performance, a relationship which has been investigated for many years, but rather in the neurophysiological advances in the understanding of the reticular formation, whose functioning seems to be consistent with activation theory. Perhaps a new model of motivational phenomena will emerge from the study of the reticular formation, hypothalamic centers and stimulation of the limbic system.

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PERSONALITY^{1, 2, 3}

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The hoary tradition that treats personality as a distinct area of theory and research was further destroyed in 1957-58. Although the idea of an inclusive category of personality will continue to have appeal in parlor and poolroom, the same is not so true in systematic psychology. As an independent, isolated compartment, personality is on its way to oblivion.

There are several places where personality is finding its contribution as an interdependent source of variance in a more systematic approach to behavior. Personality variables are being investigated in connection with hypotheses derived from comparative learning theory (107). This trend serves to bring these two categories of analysis together through a common set of concepts, with personality variables assuming the status of drives (50, 57). In such investigations, personality is seen as a factor influencing the course of learning, but the process of learning itself is not unique for any given person and therefore cannot be regarded as a component of personality theory (109).

Another example is the linking now occurring between personality theory and social psychology. This trend demonstrates that the adjustment of an individual in a group situation is related only in part to personality factors (18, 96). Cohesion, for example, retains the status of a social variable with both group properties and personality factors of individual members determining the actual degree of cohesion present in any group situation.

A third sphere of influence is typified by shifts in research design. The more traditional way of doing personality research, involving correlations of test scores of persons on different measures, is giving way to methods in which personality factors are evaluated with respect to other variables which are situational in character (8, 27, 110). Thus, it becomes more necessary to develop a way of concept formation which deals with persons and situations simultaneously. The general impact of these and other influences is

¹ This review covers the period April, 1957 to April, 1958.

² In this chapter the following abbreviations are used: ACE (Test published by the American Council on Education); F Scale (California scale for measuring authoritarianism); GSR (Galvanic skin response); MAS (Manifest anxiety scale) MMPI (Minnesota Multiphasic Personality Inventory); n achievement (need achievement); PF (Cattell Personality Factor Questionnaire); Q-sort (Method of getting subjects to sort 100 self-descriptive statements into nine piles according to a forced normal distribution); TAT (Thematic Aperception Test).

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the emergence of a way of thinking with personality variables constituting but one set of terms in a more complicated equation of simultaneously operating influences. Interaction among influences from several sources including personality becomes the focal concept. Efforts to siphon off insights with regard to personality produce an arbitrary and unwieldy mass of statements that don't fit together too well. The study of personality is blending with, and, somewhat tortuously, finding its place in a more systematic approach to behavior.

The shift is painful. The pinch is evident, at two points particularly. One is in the absence of a commonly acceptable system to guide investigators in selecting variables to be studied or in planning the conditions for investigating interactions among them. The other reveals itself in gross differences in the quality of the experimental designs through which investigations are carried out.

THEORY AND RESEARCH DESIGN

The theory that emerges from this period undoubtedly will be one in which interaction among variables representing both person and situation is central. The literature contains more examples of interaction-type investigations than previously. One by Lazarus *et al.* (69) points in this direction. They conclude, "... the traditional search for main effects of independent variables in stress experimentation must give way to analysis of interactions among variables. . . . We demonstrate that the influence of a given variable in the determination of behavior is a function of the context of other variables in which it occurs." A similar conclusion regarding interaction is drawn by Truax & Martin (115) and Shore (101).

The fact that interactions emerge occasionally appears surprising. Designs frequently are based on interaction, but the interactions investigated are not dictated by theory. Rather, they are determined by statistical requirements of, say, an analysis of variance approach. In a sense quantitative method has leaped ahead of systematic insight. The tail swings the dog, but the reversal is healthy. After this period the dog is going to regain control and go in a different direction. Interactions studied will be determined by theory, with statistical arrangements calibrated to the requirements of the interaction design.

A result from the investigation of interaction demanded by design considerations rather than by theory concerns is that variables placed in interaction often are selected more by convenience or on hunch than because they are required by a coherent system. Problems investigated by one experimenter, as contrasted with those by a second, then bear little or no relationship to one another or to theory. Often the selection is unsupported by wider implications of extending some relationship already established or testing predictions from a systematic position. Movement under these conditions tends to be horizontal along a widening front, but the front itself does not tend to move very rapidly in a forward or advancing direction. A review

of literature, such as presented here, brings the point home compellingly. Under many of the headings the studies reviewed are placed together because they bear common titles. Relationships frequently are difficult to discern and to put together in a sensible way.

There are evidences of interactions investigated to extend a theory. Further steps have been taken during the year in applying Helson's concept of adaptation-level (35, 81) in studies employing personality as one source of variance in a larger network of relationships. A similarity in selection of variables oriented with respect to one another so as to investigate interaction, but stemming from a different tradition, is found in Asch's monograph dealing with conformity pressures (4). Other developing movements toward applying theory are contained in studies which seek to utilize comparative learning theory in the analysis of human adjustment (16, 22, 84). Here the predicted outcomes have been evaluated previously at the comparative level, and the test is with regard to whether or not, or to what degree, the generalizations hold with human subjects. Rotter's learning theory is another example of work that proceeds from a systematic view (79). Two others are contained in studies designed to identify the personality correlates of behavior in the Zeigarnik interruption situation (3) and in investigations intended to test various psychoanalytic hypotheses, particularly with respect to repression (52).

Experimental designs can be evaluated from a number of points of view, some of which are presented below. Generally speaking, designs used to explore dimensions of the personality realm reveal as many individual differences as are found in the analysis of personality itself!

Conspicuous is the continued trend, represented in some 35 studies, of testing a population on a personality measure, and then of selecting and experimenting with two test samples from extremes of the distribution. The apparent rationale is that if differences attributable to personality do exist they should be demonstrated more easily by comparison of performance of extreme groups than by other approaches. Using extreme groups as "spot checks" of relationships sometimes constitutes a satisfactory first approximation, but too frequently when the same relationships are evaluated with intermediate positions represented as well, the trend turns out not to be linear and, with disturbing frequency, not even significant. A good example of how reinterpretation of relationships sometimes is required when intermediate positions are represented is presented by Mednick (84), who demonstrates a curvilinear relationship for manifest anxiety with stimulus generalization. A study by Goodstein & Farber (51) is similar in reporting that performance of individuals in the middle range of the Taylor MAS scale is better on a digit symbol task than is performance of individuals at either extreme. High-low test samples possibly constitute a satisfactory basis for pilot research, but published work would contribute more value were continuous variation to be introduced within the personality dimension.

Another consideration is with respect to order effects, either within the

personality measure itself or in the way situational factors are presented. Rather than varying order so that effects associated with it, if present, can be identified, evaluated and, if desirable, neutralized, customarily a single, standard sequence is employed. The result is that trends based on order are obscured or ignored, with conclusions drawn which fail to give proper regard to the contributions of this critical factor. One example of the importance of order is provided in a study by Feldman & Siegel (42) who interspersed items from a manifest hostility scale with items from the Taylor Manifest Anxiety Scale. Tests then were scored separately with the finding that manifest hostility scores were significantly lowered while manifest anxiety scores were unaffected by the alteration in order created through the inclusion of hostility items. Shapiro & Tagiuri (99) present a comparable result, showing the impact of order on judgment in a study of inferences from personality traits. Order effects also are shown in a study of curiosity by Berlyne (18).

Also evident is a minor revolution in the expansion of statistical methods being applied in analyzing personality data. An increasing trend is apparent in the direction of employing nonparametric treatments. This trend is healthy, but it also is one of mixed blessings. On one hand it permits investigators greater freedom in designing. Yet elegant statistics, though erudite, do not compensate for fundamentally inelegant designs.

The too common inability to replicate a study appears again. Sometimes this happens because the initial study was not reported in sufficient detail to permit a precise repetition. Other times lack of ability to confirm arises from the explosion into awareness of variables which were not known to be affecting behavior in the initial investigation. Sometimes failure to corroborate is attributable to a disregard of relatively minute but nonetheless clearly reported conditions in the initial investigation. Nevertheless, the trend toward reporting negative results under conditions of replication seems to be increasing and is to be applauded.

An important characteristic of research developments in the personality area is with respect to the impoverished situational contexts in terms of which personality variables are evaluated. Although the motivation for doing so can be understood as an effort to establish simple and relatively unidimensional situations, one is struck by the impoverished, relatively meaningless nature of the situations into which subjects are placed and asked to perform. A number of the situations used to assess the operation of an achievement, as an example, do not easily appeal to subjects as "challenging." The intrinsic properties of the task are so unhuman or mechanical as to provoke a minimum of interest, personal commitment, or striving. The low point is reached when a report indicates that subjects differing in some personal characteristic were paid a dollar an hour to engage in digit substitution or to cross out Xs with significant differences not obtained!

An important problem is connected with the impoverishment of the situations within which personality variables are studied. It is that the trans-

lation of findings for interpreting adjustment in rich life situations is difficult to envisage. As time goes on it can be anticipated that the variables governing behavior in every-day types of situations can be identified and subjected to experimental manipulation along with personal variables. Then the criticism of personality research as vacuous will no longer hold.

PERSONALITY TESTS

Two additional features of the year's efforts in the personality area are worthy of comment. One is the continued, perhaps increasing, trend toward employing a variety of un- or semi-validated instruments for the measurement of personality characteristics. An investigator begins by deciding that some personality characteristic is pertinent in accounting for an aspect of behavior. Without a standard instrument available to code individual differences, he constructs his own, according to logical considerations and on *a priori* grounds. The test then is placed in the design as an index of the personality attribute to be varied. Failure to confirm the hypothesis under study is difficult to interpret. The failure may be caused by inadequate test construction, by use of an invalid measure, by unsound hypotheses, or by a poor design. These kinds of studies frequently end with the consumer plug, "Results obtained were not as predicted, but they open up a new area. They constitute a challenge to others. . . ." Negative results from such investigations are worth little more than the paper on which they are reported, for they provide insufficient direction to other investigators who may be interested in the problem and who want to carry the research forward. The only option is to start again from scratch.

Another characteristic is the trend toward concentrating measurements on a smaller number of standard instruments. Included among the more popular measures are the McClelland achievement, the Taylor Manifest Anxiety Scale, the California F Scale, and the Cattell 16 Factor Personality Test. Results from use of these tests are reported below. Relative newcomers, gaining in popularity, are Kelley's Role Repertory Classification Test (72, 77), the Edwards Personal Preference Scale (53, 66, 102), and Osgood's Semantic Differential (24, 100). An old stand-by, the MMPI (105, 106, 112), may be going down a bit, but it continues strong.

Difficulties in employing certain of these measures will be reported below, where findings based on their use are reviewed. From a general point of view, however, the Taylor Manifest Anxiety Scale seems to be in trouble. Is it an adequate measure of anxiety as a drive? Uncertainty surrounds the California F Scale. Does it assess authoritarianism, acquiescence, intellectual sophistication, all or none? The Role Repertory Classification Test seems to provide a stable index of cognitive complexity-simplicity, but it is too early really to know. A consequence is that many otherwise excellent experiments are hamstrung for want of an interpretation of findings. What "threw the curve" into the results? Was it thrown by poor hypotheses, by a test which measures something other than what it is reputed to describe, or

because it really measures nothing of psychological relevance? Trudging at the front lines of personality test construction is much needed. The resistance of personality to systematic evaluation will be overcome only when adequate instruments are available and are in the hands of research oriented theorists.

The literature quoted in references and bibliographies is becoming more of a problem. Some of the quoted references appear in standard journals. For these sources there is no problem except that the crush of space confronting editors is causing articles to appear which contain less and less procedural and methodological information. Some literature is from obscure military documents or progress reports detailing activities within a supported research program. Here the problem becomes more serious, for not only are these materials not readily accessible, but also when they are examined it frequently is evident that the editor's touch is lacking. Publications from local sources, with limited distribution, or mimeographed materials which are unavailable to the general public constitute a plague. Students who wish to use a set of such references to evaluate the history of research, or even to check on the contents of a scale developed in some local situation, are up against it.

During the year a study of one of the difficulties with literature was undertaken. Several authors had footnoted the fact that material used in the experiment, but which was not described, was available from them, if the reader would but write. From over half those contacted, the author of the study answered with apologies, indicating that he no longer had copies available, either because the supply had run out, or because, in changing institutions, he had lightened his baggage and left them behind. With the press on publication space it is understandable that materials used in an experiment are not fully reproduced in the report and that an effort is made to tell the reader of the other sources through which the total situation can be reconstructed. Seeking to do so through mimeographed reports, however, constitutes a less desirable solution than is possible through use of the American Documentation Institute.

The remarks above, then, can be regarded as one effort to characterize the personality literature. It is tempting to say that every day in every way the field is getting better and better. A more accurate appraisal, though, identifies the struggle now going on within the personality area. One part of the struggle is to become more coherent. Another is to gain the advantage of conceptual analysis for personality which is contained in a theory focusing on interaction. Another is to take cognizance of modern statistical developments and more sophisticated designs. A fourth is to construct more adequate personality tests. A fifth is to police the development of a more useful literature. All are knotty problems.

RESEARCH FINDINGS

Areas under most extensive cultivation include anxiety, the authoritarian personality, achievement, and studies of the self. For each there is a

standard measure or procedure to evaluate individual differences. Problems surrounding the test instrument are evaluated first. Numerous investigations designed to explore the ramifications in adjustment of the personality characteristics being examined then are evaluated. The final part in each section constitutes an assessment of problems in the present situation that must be solved for further progress to be made.

Anxiety.—The topic of anxiety holds a prominent place in the minds and hearts of personality psychologists. As much effort is devoted to exploring the ins and outs of the anxiety problem as is extended on any single topic. Results for the present year's work and those from previous years, however, are enough to make the anxiety-oriented psychologist anxious over the topic of anxiety!

The basic reason is that anxiety research doesn't add up very fast, but it certainly does multiply. By this time it is reasonably clear that a valid measure, which permits the anxiety dimension of personality to be employed as an estimate of drive, would provide a most useful research instrument. Jessor & Hammond (61) and Hill (57) provide critical studies indicating that the Taylor MAS is not it.

Anxiety measures, as with other indices of personality, do not always correlate with what they are supposed to. They sometimes do with what they ought not. Questions have been raised about the Taylor Manifest Anxiety Scale, and a series of studies of the test have appeared. Unfortunately, the studies themselves do not permit unequivocal acceptance or rejection of the measure.

The problem of exploring the nature of anxiety through factor analyzing a series of measures which previously have been reported to be related to anxiety has been confronted by Martin (82). He contends that every study using psychometric anxiety as a variable must assume that there does indeed exist a dimension of individual differences which has some stability and generality and which can be measured reliably and validly. Included are paired associate learning, body sway, verbal maze learning, motor tasks, food dislikes, cancellation problems, multiplication tasks, scrambled sentences, the Try Scale, and the Taylor MAS. Eight factors are extracted. Only one, defined partly, but not substantially, by loadings from the Taylor Scale, is interpreted as an anxiety dimension. The conclusion is drawn that individual differences in anxiety level account for a relatively small per cent of obtained scores from these several techniques, all of which, singly and independently, have been used as anxiety measures. Another factor analytical study, this time within the Taylor MAS, is reported by Bendig (14).

An approach of the same general character as that by Martin is made by Wiggins (122). He reports an absence of relationship between the Taylor MAS scores and number of blocked associations in the Word Association Test in his sample of night school psychology students. Since inability to report an association has previously been shown to be symptomatic of anxiety the failure to confirm anticipated associations suggests either that

one or the other of these two indices is not a measure of anxiety or else that anxiety has multidimensional properties. Goodstein & Farber (51) also fail to find a relationship between the Taylor MAS and performance on the Wechsler Digit Symbol Test, another accepted test of degree of anxiety present.

A different approach seeking to establish a basis for interpreting anxiety measures is to test the degree of psychometric anxiety present and to relate this variable to skin conductance, on the ground that this physiological index also is a measure of anxiety. Silverman (103) reports that the Taylor MAS is not found to be related to conductance during a rest period. A task period, with or without shock, also showed no relationship between conductance and test scores. However, the Heinman forced-choice version of the Taylor MAS is reported to be directly related to conductance during rest and inversely under threat of shock. The latter result indicates that high anxiety scores on the Heinman Scale are associated with a low arousal threshold. Since the Heinman Scale is a forced-choice method which removes, or at least diminishes, test-taking attitudes from the Taylor MAS, the implication is that a purer measure of anxiety is provided by the Heinman edition than by the Taylor MAS version and that the modified measure of anxiety provided by the Heinman Scale does provide an index which to some degree parallels physiological indications of anxiety arousal. A short forced-choice revision of the Heinman version of the Taylor MAS has been described by Christie & Budnitzky (29)!

A basis for interpreting psychometric anxiety scores is provided by Levy & Kurz (73) who relate Taylor MAS scores to color and form perception in the Rorschach. Since color provides an index of emotionality, a relationship between the Taylor MAS and color responsiveness is expected. They find it. They also report that structural factors in Rorschach responses predominate for less anxious Ss.

Based on these several results, the outcome of whether or not the Taylor MAS constitutes an adequate psychometric measure of anxiety is a draw. For the present you have to put your money and take your choice, a conclusion which only repeats work reported in previous years.

Now to a review of the work in which the Taylor MAS and other indices of anxiety have been accepted at face value and used in association with other measures of adjustment for the purpose of extending understanding of personality. By every rule in the book, anxiety ought to be related to performance under stress. Truax & Martin (115) examine this possibility. They report that after failure on a simple task, greater improvement is made by Ss who are high rather than low in anxiety, as measured by the Taylor MAS or by the Stroop Interference Test. Their results are interpreted as meaning that failure excites either greater effort or greater learning in high than low anxious people. They also report that severe criticism of failure performance by the experimenter produces greater improvement than does mild criticism but only in the low anxiety group. These findings suggest

that high anxious Ss are aroused to action by failure, but Ss low in anxiety only move toward greater effort when failure has been reinforced by severe experimenter criticism.

Anxiety ought also to be associated with the tendency to assimilate negative personal information such as failure or criticism. This possibility is explored by Hunt & Schroder (60) and by Schroder & Hunt (97). They confirm that Ss high in Taylor MAS anxiety earn lower assimilation scores. An interpretation of this result is that high anxious people don't "read" negative information in the same way that it is evaluated by Ss lower in anxiety. The high anxious approach is to deny or become aggressive against the source of criticism.

Another approach seeking, but failing, to link anxiety into a network of functional relationships is by Geertsma (50), whose study is an effort to connect anxiety with judgmental, attentional, and thinking processes. He investigated time-order errors with respect to judgments of hurtfulness regarding electroshock. With Ss in high and low groups, as distributed by Taylor MAS scores, no relationship is obtained between anxiety and time-order effects.

Low, medium and high anxiety groups on the MAS were used by Shore (101) to investigate the effect of effort, induced muscularly, and anxiety regarded as a drive level on visual recognition of tachistoscopically presented targets. He finds that Ss with high MAS scores show an increase in perceptual efficiency with muscular effort. Those with low MAS scores show a slight decrease in efficiency. Shore offers the interpretation that rather than measuring drive level, the MAS may have cue properties which arouse anxiety in S toward which he responds in a manner characteristic of the way he handles stress situations. Thus Ss who are low on the MAS react to increased stress by blocking out the environment. In this way they raise their perceptual thresholds. An opposite interpretation is implied in the study by Hunt & Schroder (60).

Using psychometric anxiety as an index of drive, a number of investigations have aimed at relating anxiety to learning theory. With extreme Taylor MAS groups Mednick (85) reports heightened conditioning responsiveness, mediated generalization, and resistance to extinction for the high anxiety group. Since his results confirm earlier work, they lend credence to the contention that anxiety may influence learning. In a well designed study by Stevenson & Iscoe, reported during the previous year (108), superiority of low as compared with high anxiety Ss in the discrimination of intermediate size was obtained. Bendig & Vaughn (16) replicate the experiment, but fail to confirm their results.

In studies in which anxiety is unrelated to cognitive processes, do the results mean that the cognitive processes involved here are not influenced by anxiety, or do they mean that anxiety is inadequately measured by the Taylor MAS in these instances? The interpretation is indeterminate.

Wiggins (122) relates anxiety to problem-solving, and he reports that,

with intelligence held constant, a significant positive relationship is found between anagram performance and both the MAS and blocking on the Word Association Test. High problem-solving ability is related with high anxiety, but, as reported earlier, these two anxiety measures are unrelated to one another! Placing such results in a coherent framework is somewhat difficult.

Non-Taylor approaches to anxiety also are possible. A few have been reported. One is by Trapp & Kausler (114), who indicate that the Mandler-Sarason version of anxiety is unrelated to level of aspiration-type goal-setting behavior. Treating anxiety as a momentary state, rather than as a residual property of an individual, Dibner (36) created two interview schedules to measure it. One, the ambiguous situation, gave Ss few guidelines for selecting their responses. In the other, or structured condition, the interviewer took an active part by guiding the discussion. A general relationship is obtained between anxiety as evaluated by several methods and the ambiguity associated with the interview situation. Consistent with previous work, the generalization is that ambiguity provokes anxiety.

The problem has been turned around in a study by Block (21) who seeks to characterize personality differences as a function of individual differences in GSR. He presents evidence that highly reactive subjects on the GSR are more dependent, dreamy, idealistic and suggestible, whereas nonreactors are relatively cool, evasive, opportunistic, and independent. GSR lability is thus to be regarded not only as an index of whether an individual is anxious in a specified situation, but also as dependent on personal characteristics. Hoffman (59) has constructed a conformity scale from F scale and original items and has shown that GSR-measured anxiety is more when subjects high in test conformity are confronted with a divergent norm from which they maintain independence. The three studies are encouraging, for they support the intuition that anxiety is a relevant personality dimension which determines the conditions of adjustment in predictable ways.

Taken as a subject-matter area of personality psychology, the materials above demonstrate the importance of anxiety as a dimension of adjustment. Critical problems must be solved before a satisfactory laboratory-based theory of anxiety and its effect on adjustment is developed, however. One is with respect to the construction of one or more valid anxiety measures. Without an adequate instrument interpretation of results from experimental work is rendered extremely difficult. A second need is to test the effect of anxiety on learning in richer, more life-like, laboratory or real life situations. Another is to devise more relevant, meaningful stimulus problems through which to investigate interactions between anxiety and perception. A final problem is concerned with determining how anxiety influences problem-solving, when the problems to be solved are more complex and realistic than many currently in use.

Authoritarianism.—Authoritarianism as a personality dynamic continues to prompt much interest and effort. Past interest is revealed in an excellent bibliography of the literature by Christie & Cook (30) reporting a total of

230 titles. As with anxiety, the core problem is at the level of primary measurement. Is the California F Scale measurement of authoritarianism clean or does the acquiescent fallout so contaminate the score as to make its continued use hazardous? Needed is a California counter to answer the question as to whether the acquiescence fallout is a myth, negligible, or of serious proportions!

The problem of measuring authoritarianism by the F Scale focuses on the following issue. F Scale items are formulated in affirmative terms. To agree with them gives a high authoritarian score. But to agree with affirmative items also is a response set. Thus a high authoritarian score may mean authoritarianism or it may mean acquiescence. The problem is complicated further by the inviting suggestion that acquiescence may be a property of authoritarianism. Gage, Leavitt & Stone (49) confirm the presence of the acquiescence factor and say it is a component of authoritarianism. Bass (11) says it isn't and regards acquiescence as a significant component of the F Scale which is unrelated to authoritarianism. Cohn (32) leans toward the view that low F Scale scores constitute evidence of intellectual sophistication. All are based on empirical work. How could a more intriguing possibility be thought of as a basis for endless polemics? Humdrum as it may be, the solution eventually will come from testing arrangements and experimental procedures which permit the issue to be dealt with in concrete, operational terms. Some are trying to resolve the matter that way.

Christie, Havel & Seidenberg (31) have confronted this problem during the period, as have others in the past, by adopting the following constructive approach. They contend that acquiescence is not a part of the authoritarian style, and if it is present in the F scale, then the logical thing to do about it is to redesign the scale and to diminish the degree to which the score measures acquiescence or, even better, obliterate acquiescence entirely. One way to do so is to reverse items. But are they reversible? The answer seems to be, "Not fully, but somewhat." They develop reversals for some items but not for all and establish a reasonable interpretation of authoritarianism for those who answered the items in the authoritarian way. A second question, also dealt with by Christie, Havel & Seidenberg, is concerned with whether the acquiescence factor is an important component on the F Scale. A short subscale with a matched form of reversible items was used to examine this question. Here the answer is, "Not significantly so, but that really depends on properties of the group answering it." College undergraduates show more acquiescence than other sample Ss, like Washington lobbyists, for example.

Counterbalancing the evidence above is a new report contending that response set, not authoritarianism, *per se*, is critical. Carey, Rogow & Farrell (25) detail a study in which 4 groups of Ss rate aphorisms for agreement. They conclude that the F Scale score may well measure agreement with values expressed in an authoritarian manner but that these values may or may not be consistent with an antidemocratic attitude.

Continuing his studies of factors influencing F Scale performance, Cohn (32) favors the third view. Rather than measuring potential fascism or antidemocratic attitudes, the score may reflect intellectual sophistication. Davids & Eriksen (34) draw a similar conclusion.

Kerlinger (65) has had some fun reinterpreting a line of argument used by Bass (11) and by Messick & Jackson (87). He points out that when authoritarian scale data are factor analyzed, the decision concerning rotation is the factor analyst's. Depending on the decision, he can come up with either of two conclusions: that the major proportion of variance is due to authoritarianism and the minor to acquiescence or that the major variance is due to acquiescence and the minor to authoritarianism! That really helps! The problem is developed in a different direction by Gage, Leavitt & Stone (49) in a study which supports the view that the meaning of acquiescence resembles that of conformity, low ego strength, and low intelligence. So, though acquiescence is there, it is all right, because it is part and parcel of authoritarianism.

Thus the outcome for F Scale authoritarianism remains in doubt. The circle is completed. At this time it's dealer's choice. The options are that the F Scale is a measure of authoritarianism, that it's an index of response set, that it's a score reflecting intellectual sophistication or that it's a multi-dimensional score with contributions from each of these and possibly other sources as well. So be it. Out of sight is out of mind. Back to work. Grant the assumption that the scale is a valid measure of authoritarianism and find out how it influences adjustment.

Rosenberg & Zimet (93) report in a study of aesthetic choices that high authoritarians choose familiar and commonplace designs, while subjects low in authoritarianism show sensitivity to and acceptance of the perception of deviant forms. Rudin & Stagner (95) found people answering the F Scale in the authoritarian direction are more likely to be influenced by ground in the perception of the figure than are those answering in the less authoritarian direction. Authoritarianism again is found to be related to intolerance of ambiguity, this time by Milton (90) who shows that trials required to establish a personal norm under autokinetic conditions are fewer for high authoritarian than for low authoritarian Ss. Davids & Eriksen (34) fail to duplicate the finding when intolerance of ambiguity is measured from auditory materials. It takes real tolerance of ambiguity to live with ambiguous results such as these. Some investigator (it is impossible to predict from the evidence whether he will be a high or low authoritarian) is bound to try to clear this situation up.

Rigidity continues to be examined in a flexible manner. No evidence of calcification or rigor mortis here. If anything, the opposite is true. The predicted association is one in which subjects high in authoritarianism are expected to show more evidences of rigidity. Honkavaara (58) goes so far as to claim there is, at present, no adequate measure of dispositional rigidity, but Rehfish (92) publishes one. An association between authoritarianism and rigidity, as measured by the tendency to maintain an already

established norm under conditions tending to disrupt it, also is reported by Milton (90), but only for situations involving ego, as contrasted with task, orientation.

The anticipated relationship between reactions to authority and conformity pressures is confirmed, with Beloff (13) reporting greater conformity under simulated social pressures for high F Scale scorers. The problem of interpreting Beloff's results is fascinating and points to a general difficulty within this area. If the material presented earlier on the meaning of an authoritarian score is not yet out of mind, the findings may mean that acquiescent people yield to social pressures. That's reasonable, isn't it? But it's too simple, because if acquiescence is part and parcel of the authoritarian orientation, then the better conclusion is that authoritarian people are cowed by social pressures. That's reasonable too, isn't it? There is another way. Regard interpretation of the F Scale score under the projective hypothesis for a moment. Then one can embrace whichever interpretation of results he needs. Or, under the schizoid hypothesis, the best interpretation is that both factors generate submission to social pressures because everything is related to everything else. Perhaps next year will provide a more definite answer in the sense of removing the projective and schizoid elements from interpretation of F Scale scores.

To summarize the present status of authoritarianism as a personality variable is not easy. Problems at the level of primary measurement are of the following character: Does the authoritarian scale measure authoritarianism, acquiescence, intellectual sophistication, or a composite of factors? At the level of relating degrees of authoritarianism to aspects of adjustment the problem is no less difficult, for the reports reviewed indicate that high authoritarian subjects are intolerant of ambiguity but also that they are not. High authoritarians acquiesce to conformity pressures, but the explanation here may be in the acquiescence component of the authoritarian scale or it may be in "true" conformity susceptibility. Finally, Ss high in authoritarianism show greater rigidity, but the finding is confirmed only under conditions of ego-orientation. It can be anticipated that the psychologist's need for closure, which is not satisfied by the present state of affairs, will stimulate much new work on the problem of the authoritarian personality. To be feared is that satiation will be complete before the problem is solved.

Needs.—The need of the year is achievement, but affiliation, dependency, power, and approval are crowding achievement for popularity. As of this reading the impression is that the measurement of *n* achievement is in less trouble than are the anxiety or authoritarianism measures. Achievement possibly leads a more celibate life than many needs, avoiding other more hidden and nefarious associations. But you can never be sure. Indeed, a prediction here is that *n* achievement and other needs assessed from fantasy materials will increasingly be under attack at the methodological level. For the moment, and until the results by Lazarus *et al.* (69) and by Krumoltz & Farquhar (68) are digested, *mum* is the word.

Evidence already is mounting that measurement of needs from fantasy requires refinement. First with regard to *n* achievement. Questions are being asked regarding its validity. Lazarus and associates (69) put the issues as: (a) to what degree are motive states necessarily reflected in TAT fantasy and (b) is there always a direct and positive relationship between the expression of a need in fantasy and its actualization in behavior? Concluding from a study dealing with these questions, they suggest that the *n* achievement score needs to be reexamined. Subjects graded in *n* achievement were required to reproduce paragraphs originally experienced under auditory presentation. Paragraphs reproduced were evaluated for being literal or conceptual. High *n* achievement subjects produced more literal reconstructions than did subjects low in *n* achievement. However, when the quality of the reconstructions which were primarily conceptual was examined, no loss in understanding was evident. Furthermore, no evidence of less effort was apparent. They conclude that *n* achievement is not a pure measure of motive state and contend furthermore that it constitutes an index of an individual's style or method of coping with his world, with a high *n* achievement score associated with a lower level of intellectual functioning in terms of a maturity criterion.

An additional problem is focused by Krumboltz & Farquhar (68) who report a nine-week test-retest reliability of .26. They show also that *n* achievement is unrelated to scholastic aptitude as measured by the ACE. Bendig (15) demonstrates that fantasy *n* achievement is essentially unrelated to *n* achievement as evaluated from the Edwards Personal Preference Schedule. All in all it appears that as *n* achievement enters a new phase of the cycle in which, as more questions are being asked regarding its reliability and validity than heretofore, negative results are being obtained with embarrassing frequency. Description of a new book, *Motives in Fantasy, Action, and Society* (5), has just appeared. The blurb asserts that experimental work is presented, indicating that, "... individual differences in the strengths of motives can validly be inferred through content qualities of thematic apperceptive stories."

Studies dealing with aggressive fantasy from the TAT are reported by Lesser (70). Using a sociometric index of aggressiveness with ten 13-year-old boys he reports a significant correlation between overt aggression scores and fantasy aggression scores and a negative correlation between overt aggression scores and a measure of fantasy anxiety which blocks the expression of fantasy aggression. The correlation between fantasy aggression and overt aggression also is found by Lesser (71) to be greater for boys for whom there is greater maternal encouragement of aggression. While these studies do not parallel the one by Lazarus *et al.* (69) since fantasy aggression rather than *n* achievement is dealt with, they do provide relevant information regarding the complexity of the connections between fantasy and performance. Furthermore they support the view that regulatory personality features intervene to determine whether or not direct expression is predictable by simple knowledge of fantasy aggression. Taken together and

added to findings from the past, these studies suggest that needs, as inferred from fantasy material, merit much more intensive investigation at the methodological level than they have had before extensive use to investigate problems of behavior and adjustment is justified.

With regard to *n* achievement as a variable in investigating behavior the following findings are representative. First is the matter of the relationship between *n* achievement and maintenance of independence in conformity-evoking situations. Krebs (67) demonstrates such a relationship, thus confirming the earlier findings of McClelland *et al.* (78). Wertheim & Mednick (121) report that *n* achievement correlates significantly and inversely with scores on the Embedded Figures Test (EFT). The obtained relationship is evaluated in terms of McClelland's and of Witkins' interpretations of low *n* achievement and of perceptual field dependence respectively, with the implication drawn that both kinds of behavior in adults originate in parent-child relationships which are not demanding of independent behavior. A further finding by Bieri & Messerly (19) demonstrates greater speed in locating embedded figures for extratensive subjects. The earlier finding by Linton (74), showing that subjects who have difficulty with the EFT are high in conformity in the autokinetic situation, implies that a cluster of factors involving *n* achievement, field independence, social independence, and child rearing practices eventually may be identified.

However, Samelson's (96) report on personality correlates of conformity complicates the situation. Using a modified Asch-type situation involving presentation of nonsense syllables under tachistoscopic conditions, the experiment confronted Ss with either a full conflict situation or else with a reduced conflict situation, the latter by allowing naive Ss to gain the impression that their recognition thresholds were lower than for others present. Conformity was not found to be related to *n* affiliation, to anxiety, as measured by a modified Cattell 16 PF, or to intelligence, assessed through the ACE. In the full conflict condition *n* achievement and social approach assessed from the Cattell 16 PF are *negatively* correlated with conformity. So far so good. Under conditions of reduced conflict, however, both of the latter measures were, unexpectedly, positively related to conformity. The plot thickens. Apparently the relationship is not simple, but rather it is conditioned by variables present in the conformity evoking situation with which *n* achievement interacts.

The relation of achievement motivation to problem solving has been investigated by French & Thomas (48). They show that Ss high in achievement motivation work longer and are more likely to reach a solution than Ss with low achievement motivation. On the other hand, Vogel, Baker & Lazarus (118) demonstrate that after failure high *n* achievers attempt fewer performance items than persons of lower *n* achievement. Whether or not *n* achievement predicts performance is shown by Birney (20) to be in part a function of the strength of picture writing cues in the stimulus situation, with a positive relationship between *n* achievement scores and task performance for conditions in which a student was the experimenter

but not when faculty members served as Es. Here again is evidence that results obtained are determined by interactional circumstances. Miles (88) relates achievement drive and modes of task approach to individual differences on a learning task. High *n* achievement individuals who had been equated on the MAS, in contrast to low *n* achievement individuals, had larger losses from proactive and retroactive practice effects. The implication is that under certain conditions a high need achievement can get in the way of learning.

Where does the achievement motive stand? As far as this year is concerned it is not on the critical list, but its health is by no means improving. Embarrassing questions are being asked as to whether it's a pure dimension that varies directly with other items of behavior, and disturbing evidences of low reliability are offered. For need achievement, as with anxiety and authoritarianism, it is almost as though for every study which reports findings in one direction, there is another with opposite results.

Can *n* dependency be relied upon as a measure of dependency needs? Fitzgerald (45) reports that it cannot. He finds that TAT based *n* dependency predicts neither interview nor sociometrically measured dependency in a straight-forward and simple way. Here again is important evidence that fantasy-revealed needs do not necessarily protrude and reveal themselves at the level of performance. Incomplete sentence *n* dependency, on the other hand, is related to both the interview and the sociometric measures of dependent behavior. However, importance of conflict in *n* dependency fantasy, assessed from interviews, is related to TAT *n* dependency. The latter result specifies a variable within dependency measurement that may be of importance, and it provides the suggestion that the measurement of *n* dependency also merits purification. Since interview assessment of *n* dependency and sociometric measures relate nicely with one another, but since neither relates satisfactorily with TAT *n* dependency, a conclusion can be drawn that it's safer to stay at levels of analysis which are closer to actual behavior. Trying to assess dependency by fantasy behavior is to go about the job in the hard way.

A representative substantive investigation of *n* dependency is reported by Scodel (98). He employed *n* dependency to predict a preference of a kind that often is evaluated but not too frequently studied systematically. A significant positive correlation is found among college students between TAT *n* dependency and preference for small breasted women. Preference judgments were not taken in real life situations, but were from decapitated photographs with limbs unpruned! Results are said to be contrary to a widely held Freudian hypothesis, but they are consistent with a reinforcement theory of learning.

Personal security, measured by the Maslow S-I Inventory, has drawn some interest. The idea is that personal security ought to be a variable of importance in adjustment. In one study, Bennett & Jordan (17) inquire regarding the correlates of frustration in the insecure person. They report insecure persons to be significantly more extrapunitive with secure persons

more impunitive. Other differences between secure and insecure persons are reported by Foulds (47). This study involves an investigation of relationships between two tests, and as such it is based upon an approach to personality research which was popular some time ago. Significant support is reported by Ainsworth (1) for the idea that insecurity is related to rigidity in general life adjustment.

Self-evaluation.—The veil of anonymity was finally stripped from the self a decade or more ago under the press of Rogerian nondirective client-centered curiosity and of Hilgard's presidential address before the American Psychological Association. Today the self stands naked for all to examine, evaluate, theorize about, and try to change. It is exposed to public examination by the self evaluating itself, in situations with others present or absent, under various forms of criticism, and so on. This invasion of privacy has been accomplished with skill and taste. Personal anonymity has been protected while asking individuals to describe their selves. Apparently no individual has yet been pushed so far as to invoke either the First or the Fifth Amendment. Results are encouraging.

First to be examined are those investigations dealing with the measurement aspect. Factors determining the degree to which an individual discloses information concerning himself are reported by Jourard & Lasakow (62). They show that amount of self-disclosure varies with characteristics of the target person and category of information being assessed. The next question is, how do the various parts of the self fit together? In a study dealing with intercorrelations between self-ideal, actual self, and social self-scales, Martire & Hornberger (83) report high and significant correlations among the three scores for normal men. Correlations for women are lower, but still significant. Smith (104) reports the intercorrelations between six self measures of discrepancy; between self and ideal self; between self and social self; between social self and ideal self; and instability of self, ideal self and social self. Reliability coefficients are satisfactory, and psychological meanings currently accorded these measures are specified. Finally, relationships between self-concept measures and other indices of adjustment are reported and interpreted as indicating sensible, predictable connections between the self-concept and other psychometric test measures of adjustment.

Questions regarding relationships between self-scores and aspects of adjustment have again been asked. A sampling of results is reported here. Correlations between parental evaluations and children's self-evaluations are found by Helper (56) to be small but consistently positive. Eastman (40) measured self-acceptance and acceptance of others and found these variables related positively to marital happiness. Self and other evaluations also have been studied by Alfert (2) who finds that ideal congruent traits are attributed to others to a greater extent than are ideal discrepant traits. Stotland & Zander (110) examine changes in self-evaluation under atherapeutic laboratory conditions. Only Ss who previously had given high self-evaluations of their abilities on a performance problem lowered their

evaluations more after failure when they were observed by an expert than by a nonexpert. Wylie (123) relates self concept measures to defensive behavior, showing that defensiveness is a function of discrepancies within the self-concept or discrepancies between the self-concept and the self-ideal, or both. Cowen *et al.* (33) report more adequate self regarding attitudes for subjects low in Taylor MAS.

Self-evaluation procedures and theory have received considerable attention in the past decade. The central quality of the concept, the ease and efficiency of measurement possible by *Q*-sorts and the like, and the accessibility of changes of self-assessment under laboratory conditions and during counseling or treatment insure continued concern with the self over the period ahead. Studies such as those by Wylie and by Stotland & Zander indicate that variables in this area are characterized by circumstances of complex interaction with other variables. Thus, the various dimensions of the self are not so transparent as a simple reinforcement theory might suggest.

Ego factors.—A potpourri of ego-oriented studies contribute to the complexity of the personality literature. Labels like ego-control, ego-strength, ego-defense, and so on are used to identify the central variable of investigation. From a general perspective these studies have little in common, save their use of ego terminology and the fact that standard measures of ego-whatever rarely are used. A result is that studies in this area enjoy an advantage of having to be more deliberate about the operations by which the ego-variable is created. Such a generalization is, of course, too coarse. Some use standard devices, but under specified conditions, and some use them in standard ways. Nonetheless, the generalization serves to distinguish the ego area from other domains.

Using a complex index of ego-control with children in a nursery school situation, Livson & Mussen (75) report significantly less overt aggression for those high in ego-control. They fail to confirm an anticipated relationship between ego-control and dependent aspects of behavior, however, and explain the absence of association as due to greater socialization pressures against aggression than against dependency. In other words, since dependency is legitimate and aggression isn't, those high in control contain aggressive impulses; but generalization into the area of dependency seems not to occur.

Another study confirms an impulsivity-inhibition hypothesis: Dunn, Bliss & Siipola (39) used a word list given under five conditions as an indication of individual differences in responding either as stimulus-bound, with short reaction time, more contrast associates, less awareness of intervening association processes, or as subject-bound, with longer reaction time, more adjective-noun associations, and more frequent awareness of complex intervening associative processes. Subjects scoring high on the Guilford Inventory of Factors STDCR on impulsivity-inhibition were significantly more stimulus-bound, as predicted. No significant differences were found for Ss high and low on scales of introversion and extraversion. This lack of significance is attributed to the fact that the scales are more indicative of maladjustment

than of introversion-extraversion *per se*! With the Allport Vernon Scale of Values, Ss scoring high in the economic area are consistently more stimulus-bound, those with predominantly aesthetic or religious values more subject-bound, while Ss with the other values fall in between the extremes.

Twain (116) demonstrates, in a factor analytic study of a battery of 16 tests selected to correspond to aspects of behavioral control or impulsivity, that the analysis revealed the operation of more than one factor underlying the variables under study. The six factors accounting for control versus impulsivity are: flexible motor control, physical status, positive progressiveness, tenacious self-control, aggressive instability, and one which shall remain nameless because it was not given a title.

Excellent work continues to give distinction and direction to the Zeigarnik interruption-completion problem, a fascination in psychology for more than a quarter century. Well, it should be. Mystery stories, thrillers, "whodunits" and whatnots all seem to capitalize on the need for completion. There is no particular reason that psychologists ought not try to examine the reason why a task once begun wants completion. A better formula than is currently available as a basis for insight into the "whodunit" hunger, the disturbance at being interrupted, heightened or lowered recall of the interruption, and need to return to the activity, is much needed.

Alper (3) concludes that, working under ego-oriented conditions, strong ego Ss recall more completed tasks as test conditions become more threatening to self-esteem, whereas weak ego Ss recall more interrupted tasks under the same conditions. In a second study she evaluates and discusses the strong ego pattern in relationship with the low *n* achievement orientation reported earlier by Atkinson (6) and Atkinson & Raphelson (7).

Katz, McClintock & Sarnoff (64) relate ego defense to attitude change based on influence attempts through giving self-insight into the mechanisms of repression and projection as a basis for prejudice. Measures of ego defensiveness were taken from the TAT, a Multiple Choice Sentence Completion, items from the MMPI, and the F Scale. The influence procedure consisted of presenting a case of how the dynamics of scapegoating, based on projecting and compensating, relate to the development of anti-minority attitudes. The hypothesis that the middle group of ego-defenders are most susceptible to influence procedures was not supported by two indices but was by others, including *Pa* from the MMPI and the modified F Scale. The study is an illustration of difficulties of measurement and of problems involved in the use of extreme groups.

Kalis (63) has investigated the hypothesis that performance in a size perception experiment, involving periscopic tracing, reflects the ego differentiation and adequacy of the subject as measured by the Gough Adjective Check List. Prediction is that persons with an inadequately differentiated ego overestimate size. Results support the prediction. Ss who underestimate size are characterized by adjectives indicative of harmony or congruence of self-world perceptions. Ss who overestimate size are seen as lacking congruence of self-world perceptions. The middle group is significantly higher than

either extreme group on ratings of soundness, adjustment, good judgment, potential success, poise, positive character integration, and naturalness, and lower on self-defensiveness.

Ego-oriented variables make reasonably good sense as sources of individual differences in relation with other aspects of adjustment. The sense may be more illusory than real because with each investigator using his own methods for distinguishing individuals in the ego-relevant dimensions, there is relatively little opportunity to evaluate the degree to which one experiment replicates the conditions of another.

Ego factors revealed through free figure drawing also are the object of query. A relationship between ego-defense and drawings from the House-Tree-Person Test under the following circumstances is reported by Cassel, Johnson & Burns (26). The test was administered to employee applicants with the examiner either absent or present. He reports that the examiner variable significantly affects results. A typical finding is that drawings, particularly of the tree and person, are larger when the examiner is absent.

Comparing drawings of the self by older adults, Lorge, Tuckman & Dunn (76) report that drawings by the older people show increasing loss of intactness, revealed in incompleteness, lack of integration, bizarreness, lack of proportion, and loss of sex identification. All the latter aspects can be regarded as indices of ego-integration.

A thorough review of the literature regarding human figure drawings has been provided by Swensen (111), who concludes that personality factors revealed through this instrument do not conform with Machover's hypotheses regarding their meaning. Here again is a situation in which promise has not been supported by actual performance.

An exciting new dimension of personality analysis is contained in the body image concept. The idea of relationship between body image and personality is as old as Methuselah, or at least Kretschmer, but the dimensions of analysis and method of measurement developed in the past few years by Fisher & Cleveland open the problem to more systematic inquiry. An excellent overview of the approach is contained in a book by Fisher & Cleveland called *Body Image and Personality* (43).

Additional studies have been reported during the year. Ware, Fisher & Cleveland (119) have extended the body image concept, again showing that the barrier score is related to personal adjustment. Individuals who conceive their body boundaries as possessing defensive, armoring, barrier-like qualities, show greater adaptability to circumstances surrounding physical disability associated with poliomyelitis. In another report Fisher & Cleveland (44) confirm a relationship between barrier scores and sex interest, with high barrier individuals characterized by greater sexual interest and expressiveness than subjects with low barrier scores. "Stone walls do not a prison make. . . ." The originality behind the approach to body image by Fisher & Cleveland suggests that the idea will provoke much interest as it becomes better known. If the history of research involving personality attributes can be relied upon, however, it can be anticipated with the body

image concept that interactional influences will help sharpen the statement of conditions under which body image dynamics and adjustment are inter-related.

Identification and empathy.—The psychological concept of identification has been examined from several points of view. The importance of the variable is in the fact that other people, selected as models, may significantly and differentially influence the quality of an individual's adjustment. Mac-coby & Wilson (80) investigate the effect on learning from film presentation of identification with characters who portray the action. Using seventh graders, they conclude that with a film including both a strong male and female lead, viewers will identify on a like-sex basis. Furthermore, when a choice is available, identification is influenced, not by actual social class, but by social class aspiration. Finally, they indicate that amount of learning is, in part, differentially influenced by identification. Brown (23) working with children also reports a like-sex role preference. Chambers (28) reports confirmation of the hypothesis of positive identification with photographs of people of the same sex. To provide the usual complication contained in personality research it should be reported that Beier *et al.* (12) fail to confirm this finding.

Another approach is by Milton (89) who studied relationships between sex-role identification and problem-solving skill. Using the Terman-Miles M-F and MMPI M-F scales, he demonstrates that skill in problem-solving is related to sex-role identification. Differences in performance by men and women are diminished when sex-role identification is taken into consideration. This means that the more masculine the sex-role identification, regardless of actual sex, the higher the skill in problem-solving. Thus a biological variable and a psychological variable are separated, with variance in performance correctly assigned to psychological factors. Van Krevelen (117) also reports that college Ss judging personality traits depicted in the Szondi test use traits they considered typical of themselves more frequently than traits not considered typical. While the study is one of projection, it suggests one of the mechanisms underlying identification.

Of the many topics under inquiry within the personality area, problems associated with empathy probably enjoy as much seniority as any. Foa (46) shows the critical importance in valid empathy of the degree of transparency in the behavior of the person whose behavior is evaluated. Studies of individual differences in ability to empathize need to regard this factor in the design of experiments. In another study empathy, as measured by accuracy of nurses' rating fellow group members on an 80-item inventory, was found by Halpern (55) to be positively related to the Social Values Scale from the Allport Vernon Study of Values and negatively related to the Esthetic Value Scale.

Creativity.—Creativity continues to stir the imagination. The standard approach is that of selecting extreme groups according to some creativity criterion and then seeking to characterize distinctions between them. Drev-dahl & Cattell (38) find that a sample of creative artists and writers differ

from a normal population on the 16 PF Test. Artists are characterized as more intelligent, emotionally more mature, dominant, adventurous, emotionally sensitive, Bohemian, radical, self-sufficient, of a higher ergic tension level, surgent, and less cyclothymic. They are less subject to group standards and controls, a finding which probably accounts for their long hair, goatees, and sneakers! Eiduson (41), using Rorschach and TAT measures, also reports significant differences between artists and nonartists, with artists being more original and unusual, displaying novelty in thinking, organizing ideas into unusual conceptions and so on. They also are able to tolerate ambiguity, show sensitivity to their own needs and to those of others, and establish a multiplicity of identifications. Griffin (54) asked heads of departments to select creative and noncreative people according to the department heads' own standards. Groups were then compared on the Levy Movement Blots. Differences in movement, which ordinarily is regarded as an index of creativity, were not obtained.

Originality within the normal range has been studied by Barron (9). He reports results concerning personality characteristics for differences between high and low scorers on an originality composite composed of tests from the Guilford creativity battery, Rorschach, TAT, an original set of inkblots, anagrams, word usages, and a number of other measures. With verbal intelligence partialled out, significant relationships between originality and the following groupings are reported: (a) disposition toward integration of diverse stimuli; (b) energy, fluent output, involvement; (c) personal domination and self-assertion; (d) responsiveness to impulse and emotions; (e) expressed femininity of interests; (f) general effectiveness of performance.

Taking these several studies as a unit, it begins to look as if artists are good people. They are a little like Roy Rogers or Zorro, with more of a flair for the unusual and novel. They certainly are unlike the authoritarians described earlier.

Personal consistency in conformity.—With situational aspects of conformity dynamics reasonably well understood, questions regarding contributions of personality are exciting more attention. Two kinds of issues are being raised. One has to do with describing the personality structure of the person who capitulates under social pressure. The other deals with the degree of consistency of capitulation across tasks, over a range of conditions, and through time.

Rosner (94) has repeated conditions of the Asch-type social pressure situation, with task content subject to coercive pressures involving line comparisons, memory tasks, and a questionnaire. Consistency in personal behavior is reported on a within-task basis for a single sitting, between tasks during a single sitting, and between sittings. Beloff (13) also has confronted the problem, reporting that submissive people and high authoritarians are more conforming, as mentioned earlier. Using the Behavior Interpretation Inventory to select college Ss, Moeller & Applezweig (91) tested and confirmed the hypothesis that conformity in the Asch-type situation is related

to high need for social-approval and to low self-approval motivation. As of this writing, the claim that individuals are personally consistent in reactions to conformity pressures is in good health. No known exceptions to the rule have been reported. The rule that seems to be emerging is "Once a conformer, always a conformer," rather than "Once tried, never again."

Broader personality aspects.—Efforts to identify cultural dimensions in personality development and the properties of individuals who are characterized by psychological health also have been reported. Based on a survey of socialization practices in 110 cultures, Barry, Bacon & Child (10) report a widespread pattern across cultures of greater pressures toward nurturance, obedience and responsibility in girls, and greater self-reliance and achievement-striving in boys. These findings are interpreted as confirming cultural rather than biological sources of personality differences.

Factors in child-rearing which influence personality and adjustment have been studied by Watson (120). Extreme homes in terms of permissiveness or strictness of parental control were selected through parents' questionnaires and social worker ratings. Children were assessed through free play, Rorschach, TAT, figure drawing, a performance test and school behavior ratings. Significant differences were found favoring children from permissive homes. Greater freedom for the child is associated with (a) more initiative and independence, (b) better socialization and cooperation, (c) less inner hostility and more friendly feeling toward others and (d) a higher level of spontaneity, originality, and creativity. No differences in self-control, inner security, happiness or energy-level were found.

Using survey research techniques with Boy Scouts, Douvan & Adelson (37) report on mobility of adolescent boys from the standpoint of personality structure. They propose, and support with empirical data, the idea that upward mobility is found among boys with autonomous ego functioning. Downward mobility is symptomatic of demoralization. Findings demonstrate the upwardly mobile boys are characterized by higher energy level, pervasive achievement orientation, extended time perspective, interiorized standards of personal behavior, personal autonomy, realistic self-assessment, and so on.

Experimental work which seeks to establish correlates of psychological health has been retarded by the lack of satisfactory specification of the ingredients of health. The studies below constitute efforts to deal with this problem, but it is likely that progress toward the intensive investigation of psychological health will be exhilarated once some operational criteria of psychological health have been established.

Thorne (113) orients the problem by presenting an empirical report of adults studied over a 17-year period with the following conclusions drawn: Psychological health is multidimensional. The "level" varies over time. Conventionality, in contrast to self-actualization and autonomy, may be healthy for some individuals. Five of the more important frames of measurement are: freedom from chronic mental disorder, stable work history, broad range of vocational and avocational interests, and stable conduct as reflected by

thrift, sobriety, and absence of asocial offenses. In a somewhat related study college students selected as psychologically healthy or unhealthy according to the MMPI were found by Mehlman & Kaplan (86) not to differ in terms of either self-actualization (Maslow S-I Questionnaire) or autonomy (Reisman Questionnaire).

CRITIQUE

"Gad, what a mess!" constitutes one appraisal of personality literature, which emphasizes its brittleness and the fact that results from one study frequently break and disappear under replication. This appraisal also takes into consideration the fact that the area is ridden with results from one experiment which contradict those from another. Another evaluation is that progress will be limited until more valid measures of personality parameters are available. A third is that many improvements are possible in the formulations of problems and in the research designs by which they are approached. All seem to be justified statements, yet they fail to identify the drama of present-day personality research. What are the deeper-lying concerns that motivate personality psychologists in the selection of the problems on which they work?

As a point of comparison consider the rallying cry of the French Revolution which expressed dominant concerns of that time: "*Liberté, égalité, fraternité*." A quite different triumvirate of issues is evident in the personality research of today. If published literature can be taken even as a rough indication, our primary interests are: "Achievement, Anxiety, Authoritarianism (AAA)." These three topics compel our principal involvements and effort. They characterize the primary motif, the theme of current research emphasis. A legitimate question is, "Why?" Is the AAA emphasis popular because of ease and adequacy of measurement? The answer clearly is, "No." Even a cursory examination of the measurement aspects indicates that. Perhaps some few investigators are drawn to one or another of these areas because a test is conveniently available, but a more fundamental explanation is to be found in deeper considerations.

Surely the modern social scene is a striving, accomplishment-centered, achievement-oriented one, and systematic understanding of the dynamics of achievement could contribute much toward an appreciation of this dimension of adjustment of individuals in modern culture. A possibility exists that the attraction of research on achievement is somewhat related to these considerations. Emphasis on anxiety as a research topic possibly can be understood in similar terms. Statistical summaries detail the prevalence of mental illness and milder failures of adjustment. Awareness of widespread tensions, frictions, and strains in modern living and the developing interest in mental health focus this great area of concern. A key to it may be found in an understanding of the psychodynamics of anxiety. Personality research centering on anxiety, then, constitutes but one expression within psychology of this broader cultural awareness. The third sphere of research action, concentrating on the authoritarian dimension of personality, also parallels a

cultural awareness of the problem of authority and adjustment to it. Authority is of primary significance from the standpoint of understanding a range of relationships from behavior within political systems to an appreciation of conditions conducive to change in psychotherapy. A deeper understanding of the authoritarian personality can shed light on causes of problems of adjustments to authority and suggest resolutions for them.

Of course there are secondary themes such as consistency in personal behavior and creativity, and tertiary ones, like security and evaluation of the self, but in many respects the latter problems can be regarded as versions of those depicted above. If this analysis of cultural determinism of interests is correct, it places a different light on the personality research of today, for it indicates that personality research is motivated by functional interests and, furthermore, that the problems which are being grappled with are ones of great complexity. Under these circumstances the brittle, riddled quality of personality research may be understood as reflecting the inevitable confusion of a research area in its early phases of development.

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SOCIAL PSYCHOLOGY AND GROUP PROCESSES¹

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Ten years ago, Bruner (22), reviewing social psychology for the first volume of this series, closed by saying, "The critical shortage in social psychology is not in its lack of zeal for data. . ." It still isn't. When the bibliography for this review reached 200 items, the reviewer could still laugh. When it numbered 350 items and was still incomplete, things were far from funny because almost half the pages allotted for this chapter could have been taken up by the bibliographical entries alone. While this result can give rise to some intriguing inner experiences as deadline time approaches, the solutions which suggest themselves don't really go to the heart of the problem, which is that no review of a year's output in social psychology can lay claim to comprehensiveness when it is restricted to a maximum of 30 printed pages. The only reasonable solution seemed to be for the writer to exercise ruthless selectivity in choosing what was to be included in the review. That was done. Any selection procedure is bound to be more instead of less arbitrary and unjustifiable. The one used here won't be defended beyond stating its purpose, which has two parts. They are (a) to cover extensively the areas in which the greatest amount of work has been done during the year and (b) to present material outside the major areas in terms of its apparent significance to the field.

SOCIAL INFLUENCE, AND OPINION AND ATTITUDE CHANGE

Together with the area of social perception, the study of influence processes received the year's greatest amount of attention. During the year, Festinger (47) published a major theoretical statement on the general topic of communication and social influence, as did Zetterberg (145), and no less than forty experimental articles appeared. Festinger's theory hinges on the concept of "dissonance," briefly definable as a state wherein there exist within the person "logical" inconsistencies in some cognitive and perceptual elements. The major hypotheses of the system state that (a) the presence of dissonance motivates the person to reduce it, thereby achieving consonance, and (b) when dissonance is present the person will avoid situations and behaviors which will increase it.

From these initial statements Festinger went on to more precise statements about the consequences which follow from personal decisions, forced compliance with a group norm, voluntary and involuntary exposure to information, and social support. Taken with the data offered as supporting evidence, the work is an impressive step in formalizing theory in the area,

¹ This review covers the period between April, 1957, and April, 1958.

however one might feel about the terms in which Festinger chose to couch it.

Ehrlich *et al.* (43) presented some support for Festinger's decision-making derivations. They investigated the automobile advertisement reading of new and old car owners. The theory would predict that an S would read ads for his own new car, but avoid ads for cars he had considered but not bought. Ss did read their own cars' ads more often although they did not avoid ads for the considered cars. Since dissonance is postulated as dissipating over time, the differences should not be present among owners of old cars. The last notion was sustained as well as any equivalent of the null hypothesis can be: the owners of old cars will read any old ad.

Cohen, Brehm & Fleming (33) also presented some evidence on another of Festinger's points. Specifically, they argued that a discrepant opinion seen by S as being externally justified by other needs would create less dissonance and, hence, less opinion change. The hypothesis received some support.

Berkowitz (13) also published data bearing upon Festinger's hypotheses. He established groups of either high or low attractiveness for his Ss and had them give a series of judgments on—supposedly—the same stimulus objects. There was greater conformity behavior in the high than in the low attractiveness condition. The merit of the group's behavior was also rated high in the high attractiveness situation. Berkowitz directs his major argument to the point that conformity is not necessarily the result of a fear of rejection or "pressures to uniformity." Instead, high group attractiveness plus behavior which is "wrong" may produce cognitive strain, or dissonance, which may be reduced by reinterpreting the group's behavior as "good." Hence, conformity results as a product of the reinterpretation.

Situational determinants.—Several investigators dealt with the effects of the distance variable; that is, how far a communication or group norm was from S's own position. In a refreshingly straightforward and readable presentation, Hovland & Pritzker (71) measured the extent of opinion change as a function of the amount of change advocated by groups S had previously indicated were "authority" groups for him. The amount of opinion was directly proportional to the amount advocated but not a constant percentage of it as Goldberg (57) had reported several years ago. In this study the per cent change dropped from 88 to 62 to 58 per cent as distance increased.

Fisher & Lubin (51) confirmed and extended the Hovland-Pritzker findings. After seeing for 10 sec. a photograph containing 165 stimulus figures, S gave his estimate of the number and received a fictitious estimate from his partner differing by 0, 40, 160, 320, 1300, or 5100 units. The amount of movement to the partner's estimate was a negatively accelerated monotonic function of distance, but the per cent of movement dropped to near zero when the amount advocated became "implausible." The amount of change, by the way, increased with repetition of the influence attempts and carried over to initial judgments of a second photograph. Among other

things (which will be discussed later), Blake, Helson & Mouton (17) also found greater conformity at the smaller distances.

Hovland, Harvey & Sherif (70) using the "hot issue" of prohibition (for their Ss from a dry state) found that when the communication advocating change is far from S's own position he doesn't change, while an S whose position is closer does, and in the direction advocated. The study suggested a reason for the absence of the advocated shift with more distance communications; Ss perceive them as propagandistic and unfair. Besides this, two other findings are of interest: If distance is great, Ss perceive the communication as further away from their stand than it really is and, if distance is small, as closer than it really is. The problem of distance as a variable in social influence seems on its way to showing the typical solution involving complex interactions with other variables. And that remark is not meant to be a snide aside.

Other variables of the situational sort which received attention included the old one regarding the nature of the source providing the discrepant information. Credibility or relevance of the communication source was investigated by Weiss (141), Fine (50) and Kerrick (82). The last named investigator found that relevant sources generally produced more change of opinion than irrelevant ones. Fine obtained no difference in amount of opinion change as a result of a communication attributed to a highly credible (*New York Times*) and a not so credible (*New York Daily Worker*) source. If relevance and credibility bear the relation the reviewer thinks they do, this is a pretty kettle of fish, not to mention the fact that Fine's result is not in accord with a large amount of previous work (69). In Weiss' experiment, the interest was upon the effect of congruence of one's opinion with that of a negative source (the *Daily Worker* again) on one issue and subsequent opinion change on a second issue. His finding was that a previous agreement led to more people changing in the advocated direction later, although the amount of the change was not significantly different from the control condition. He also found that to have an untrustworthy source agree with one in an opinion favorable to a topic doesn't change one's opinion on it in an unfavorable direction: there was, then, no "boomerang" effect.

Two other situational variables were studied: majority group size and ambiguity of the stimulus object. Wiener, Carpenter & Carpenter (142) and Kidd (83) studied the effect of group size and found that, unlike Asch (6) but like Goldberg (57), the size of the majority made no difference in the amount of change of opinion brought about. Kidd's study also showed that where the source of the influence attempt is identifiable, conformity is greater. On the ambiguity variable, information is provided by Blake, Helson & Mouton (17), Kelley & Lamb (79), and Wiener *et al.* (142). The first named study found that judgments in difficult tasks (which here is equated with ambiguity) are more affected by group norms than they are in easy tasks. The clever experiment of Kelley & Lamb also indicated that more ambiguous stimulus situations produced greater conformity. They

make use of the fact that there are tasters and nontasters of phenylthiourea (PTU), a bitter substance. PTU tasters were unaffected by the pleasantness judgment of the taste of the mixture made by nontasters, when it was mixed with cherry syrup, while nontasters were affected by the judgment of a PTU tasting majority. Wiener *et al.* entered a dissent on the ambiguity variable in general.

Studies also appeared on the question of whether or not discussion after receiving a communication enhances the influence of the communication. Selecting Ss from the extremes and middle of a distribution of California E (ethnocentrism) scale scores, Mitnich & McGinnies (108) showed a film which treated prejudice as a disease to two groups of Ss, one of which discussed the material for one-half hour after its presentation. Both groups took the E scale again after the film or discussion as well as one month later. A control group took the E scale at the equivalent time intervals. Both experimental groups differed from the control group in amount of change on the E scale, but there was a significant interaction between Ss' original positions on the E scale and experimental treatment due primarily to the effect discussion had upon the highly ethnocentric Ss. In the discussion, the high scorers apparently received social support for their position from one another and hence were less affected by the film's content. On the delayed test, the film-discussion group retained most of their attitude change while those who saw the film alone showed a significant shift back to their original position. Discussion is, then, more effective in the long run but may backfire if the distance between some Ss' original positions and that of the communication is great. Perhaps the reason lies in the process described by Hovland, Harvey & Sherif (70) and reported earlier.

In another experiment on the effectiveness of group discussion after the presentation of films designed to change attitudes, McGinnies, Lana & Smith (103) found that discussion made no difference in the amount of attitude change. There was no evaluation of its effects as a function of original position and no delayed test was given. However, seeing three such films was found to be more effective than a single presentation of any one of them.

As a transition from studies of situational variables to the investigations of the effects of individual differences in influence phenomena, let us examine an experiment which includes both. Mausner & Bloch (101) reported on the additivity of three variables which had been found previously effective in producing conformity to a partner: (a) S's own previous success or failure on the task, (b) whether S did or did not know of his partner's previous success or failure on the task and (c) whether or not there had been previous cooperative activity between S and his partner. Earlier results indicated that (a) a previously failing S, (b) an S with a successful partner, and (c) previous cooperation between Ss each separately produced more conformity. Mausner & Bloch therefore expected that, when all three factors operated at the same time, more conformity would occur when all three

favored it, followed in order by conditions where two were pro and one con, one pro and two con, and three con. The order came out very much as expected, but Mausner & Bloch were disappointed because the intermediate sets were not significantly different. They say the result is a bar to immediate quantification of the interaction of the three variables. If the authors mean that in a $2 \times 2 \times 2$ design with variables previously unscaled for relative effectiveness in producing change they expected to pick the correct levels by intuition and to find simple additivity among the variables, they are, of course, right. If so they must have been unduly optimistic and then overreacted to the—for them—negative result. In any event, the variables look promising as building blocks for an experimental analysis of conformity and social influence.

Individual differences.—Do conformity and susceptibility to influence make up a rather general characteristic of an individual? Four studies indicate that they do. Blake, Helson & Mouton (17) showed that Ss likely to conform to the group in judging the number of metronome clicks were also likely to do so in expressing attitudes toward war and in giving answers to arithmetic problems. Rosner (120) found the Ss who yield to group pressure on one judgmental task usually yield on others as well. Beloff (12) concurred in finding conformity a fairly generalized pattern, although some sex differences appeared. Jackson (72) presented a most interesting set of data. Selecting extreme scores on a conformity inventory, Jackson had Ss view a drawing of the Necker cube with instructions to maintain it in the "up" phase. Nonconforming Ss reported fewer reversals than the conformists. This result, indicating a rather broad pattern of resistance to change within an individual, should certainly be verified if possible.

Motivational factors.—Since conformity seems to be a general pattern in a person's behavior, what produces it? A number of studies answered by asserting that a need of some sort is at least partially responsible. Argyle (5) stated that each individual has "a need for acceptance and that people learn to conform in order to be accepted"; he also averred that this probably means more public than private conformity. Testing these predictions in a 2×2 design where the need for acceptance was or was not aroused by rejecting behavior and where attitudes were expressed either publicly or privately, he found that about one-half of the Ss conform when publicly expressing their attitude and only one-quarter when privately doing so. The need-arousal manipulation produced no significant differences.

Working with measured need for affiliation, Hardy (62) placed his Ss in a situation where their attitudes toward divorce were opposed by three of four of Hardy's confederates, either with the fourth remaining silent but agreeing with them ("no support" condition) or with the fourth active and agreeing with S ("support" condition). In both public and private statements of attitude, Ss high in need for affiliation conformed more in the no-support condition, but Ss low in the need conformed to the majority more in the support condition. Hardy argued that the reason for the peculiar

result in the support condition was due to the content of the group discussion. Certainly something other than need for affiliation would seem to have been operating.

Moeller & Applezweig (109) dealt with Ss selected on the basis of two scales, one which measured need for self approval and the other need for social approval. Ss scoring high in need for social approval and low in need for self approval made up one group, those high in both needs a second, and those scoring low in social approval and high in self approval the third. The greatest amount of conformity, it was predicted, should have been in the first group and the least in the third. The first group was more conforming in an Asch (6) line-judging situation than either of the other two groups, which did not differ significantly from one another. The last part of the finding should not be too disturbing since even equally high scale scores undoubtedly do not mean equal strength of need. This possibility alone could account for the result obtained in the high need conflict group.

Samelson (121) treated the Asch line-judging conformity situation as a conflict between conforming and one's sensory experience, and argued that Ss convinced of their own greater sensory acuity should have less conflict than those not so convinced. This prediction was confirmed. Additional data showed no significant relation between conformity and need for affiliation or anxiety. With reduced conflict only need for achievement and a measure of "social approach" yielded a significant positive correlation with conformity. Krebs (88) also related need for achievement and Ss' ages at beginning of independence training (which had an insignificant r_{bis} of .21 incidentally) to conformity behavior. Ss trained early in independence show less conformity, as do those high on need for achievement. In line with the Krebs finding of the importance of early training as a factor in conformity behavior, Mussen & Kagan (112) reported that conformists perceived parental figures as harsh, punitive, restrictive, and rejecting; they suggested that this implies that conforming responses may be learned at an early age.

Two studies seem of more than passing importance in regard to the motivational components of conformity behavior because both of them point to the conclusion that conformity may be a result of learned fear-reduction. Hoffman (67) selected Ss who were high or low in their valuation of conformity. He then found that when Ss did not conform to a group norm, those who placed a high value on conformity gave a larger galvanic skin response than low scoring Ss; he also found that Ss had a higher GSR when they did not conform than when they did. Lawson & Stagner (91), although predicting the opposite, confirmed that part of Hoffman's experiment which found that an emotional response (measured by palmar sweating) decreased when an attitude shifted in the direction of the group norm. Taken together with Mussen & Kagan's description of how the conformist sees parental figures, further more explicit investigations should be made.

Other personality factors.—Two studies were published by Lundy & Berkowitz on attitude change in relation to how a person cognitively orders

the social world around him. In the first study (98), the authors found that those who have complex cognitive structures change their attitudes toward the norms of both peers and authority figures more than do Ss having a simple cognitive structure. The measure of cognitive simplicity was Kelly's Role Repertory Test (80) and the results make one wonder about its relation to intolerance of ambiguity, rigidity, authoritarianism, and the like in conjunction with susceptibility to social influence.

In their later study, these authors (16) used the complexity measure plus the F (California authoritarian) scale and one to measure Ss' confidence in interpersonal relations; some fairly unsatisfactory answers appeared. First, those Ss who were more influenced by peers than by authority figures were low in interpersonal confidence. Second, the Ss with the reverse influence pattern had high confidence scores and either high F scores or low cognitive complexity. Third, Ss influenced equally by both kinds of sources tend to have high confidence scores and either high F scores or high cognitive complexity. The patterns are straightforward enough but the lack of association between cognitive complexity and F is puzzling. Menzel (104) furnished some data which mesh neatly from the reverse angle with part of the above finding. (Lundy & Berkowitz had found that low interpersonal confidence meant more conformity to peer opinion). Menzel argued that feeling accepted within the peer group allows for greater expression of disagreement than is possible to those low in feelings of acceptance. Using physicians as Ss, he found that low-acceptance physicians reported more up-to-date behavior in prescribing new drugs than their prescription records indicated while high-acceptance Ss seldom so reported.

Katz, McClintock & Sarnoff (76) stated that ego defensiveness should be related to changes in emotionally held attitudes when the influence attempt tries to give insight into the personality dynamics underlying many such attitudes. On the basis of previous work, they expected that (a) low ego-defenders would be least affected since defensiveness is not the basis for their attitudes and (b) intermediate ego-defenders would shift more than high ego-defenders because the latter would be more "threatened and thus heighten their defenses." Ego-defensiveness was measured by part of the F scale, the Minnesota Multiphasic Personality Inventory's paranoid items, specially devised Thematic Apperception Test cards and a multiple choice sentence completion test. After reading a booklet on the operation of defense mechanisms in prejudice, their Ss' attitudes toward Negroes changed as expected but only for the F and MMPI measures of defensiveness.

Two attempts to change attitudes by including insight via role playing produced contradictory results. Culbertson (39) had Ss with negative attitudes towards Negroes either play or watch being played the role of a Negro who was moving into a previously all-white housing area. Role players were more apt to change than were observers, and control group Ss were least likely to do so. Stanley & Klausmeier (128) reported no change in attitude as a result of role playing. Their classroom situation experiment may not,

however, have been the best circumstance under which to test the proposition.

The "brainwashing" phenomenon is one in which there is great current interest and it is surely an extreme instance of influence and attitude change. Farber, Harlow & West (45) have given a very reasonable explanation of the processes involved. Analyzing the social situations of prisoners of war they conclude that three main factors are at work: debility, dependency, and dread (DDD). Extremes of DDD were said to produce hyporesponsiveness, disrupt the time spanning process and disorganize the self-concept. The person is then lead into acceptance of new referential frameworks via exploitation of DDD as they were originally employed in the typical socialization process.

In concluding this section, reference should be made to a paper by Hollander (68). For some time now, the reviewer has had vague feelings of discomfort about the conceptual status of "conformity." These feelings never became any more clearly expressed than wondering how the devil one would go about measuring conformity when the person concerned was conforming to a norm of nonconformity. Hollander clearly pointed out that our current procedures have serious omissions and discussed the problem of establishing criteria for stating when conforming behavior is or is not occurring. He then connected the variables producing conformity to status and derived the notion of "idiosyncrasy credit," which increases as the group's perception of the individual's task success increases, thus giving higher status and greater leeway for the individual to deviate from the group's expectancies. While this solution is not completely satisfying, having the problem laid out so nicely is gratifying.

SOCIAL PERCEPTION AND IMPRESSION FORMATION

The field of social perception and impression formation continues to grow in scope until it becomes difficult to place its limits with any exactitude. With this growth, happily, there is more sophistication and less of the look-what-I-found approach of the small boy with some unpleasant object on the end of a stick. One of the reasons for this, of course, is that studies showing the effects of Cronbach's (36) astute analysis are increasing in number. Besides that, however, there are other hopeful signs. For example, Tagiuri (135) began an article by saying, "Recent approaches to the problem of (social) perception . . . with few exceptions have been empirical, highly operational, often guided by rather simple hypotheses and with emphasis upon 'accuracy.'" Continuing, the author argued that it is high time that concern shifted from correlations between the products of social perception to an examination of the process of social perception.

Another sign of the change was given by Bruner & Perlmutter (23), who warn that impression formation is not a separate sort of perceptual-cognitive activity. Vinacke (140) made the same point by regarding stereotypes as concepts with the same characteristics as other concepts and insist-

ing that process rather than content is the proper object of study.

On the straight theory side, Campbell (24) attempted an answer to the question of how we go about perceiving such invisible entities as social groups. He proposed that it is accomplished on the basis of the familiar gestalt principles of perception—proximity, similarity, pregnanz, etc.—plus the additional factors of (a) resistance to intrusion of external energy, matter or diagnostic probes, and (b) internal transfer, diffusion and communication.

Turning to some methodological considerations, Crow & Hammond (37) in a competent study emphasized the importance of response sets in judging behavior. Their results indicated that social perception accuracy scores may be seriously inflated or deflated by congruence or incongruence of response sets with the judged objects.

Shapiro & Tagiuri (123) showed that the categories into which a judge must code his responses have an effect upon impressions formed of the same stimulus object. Specifically, Ss were asked to rate the traits of people who "are intelligent and independent." A pair of critical response traits, "warm" and "responsible," were arranged in various positions among eight different sets of five other traits forming the contexts. Results for the response trait "responsible" illustrated that (a) the context made a difference in its frequency of occurrence, (b) a strong emphasis on one trait reduces the probability of a strong emphasis on others, and (c) position of a trait in the list of response traits changes the probability of an inference from it.

Steiner & Dodge (129) proved that care is needed in clearly specifying whether a judge is to rate the manifest stimulus value of the other person or whether he is to predict the other's self perception. In their experiment, the two procedures not only yielded different results but also differed depending upon the situation in which the scores were obtained—frustration or lack of frustration of the judge.

Steiner & McDiarmid (130) and Alfert (2) presented evidence that "assumed similarity" and "assumed similarity between opposites" have two components each. Alfert's evidence showed that assumed similarity is greater when the judge is rating traits where his self and ideal self are congruent than when they are discrepant, and that this difference is greater for "undesirable" discrepant traits. She argued that the judge probably has different attitudes when judging discrepant and congruent traits, and thus assumed similarity is not a unitary measure. Steiner & McDiarmid performed the same operation for Fiedler's (48) assumed similarity between opposites. They found that although elevation difference scores and profile dissimilarity scores correlate positively with Fiedler's measure, they correlate in different degrees and perhaps directions with other variables. Steiner & McDiarmid concluded that the two scores should be taken as more precise than the single one of assumed similarity between opposites.

Perception of sociometric relations.—Kogan & Tagiuri (86) made an explicit test of Heider's (65) position that cognitive structures tend to be

symmetrical (balanced) in nature by examining the perceived sociometric linkages in groups of naval personnel. (A balanced relationship would take a form like the following: if person s likes persons o and q , he will believe that o and q like each other. An example of an imbalanced form is: if s dislikes o and likes q , he will believe that o likes q .) Agreement with the theoretical prediction was high with one additional note that actual imbalanced sociometric units are themselves rare. This last observation would seem to be well worth following up. In two studies Tagiuri & Kogan looked at the visibility of sociometric choices, finding first (136) that such choices are most visible when the relationship between chooser and chosen is reciprocal, and later suggesting (87) that visibility scores are of use only with third persons who are not the object of choice. Anikeef (4) also had a note on this point in which he found that members of a small group are generally able to predict the sociometric choices which will be made within it.

Situational or stimulus determinants.—Jones & deCharms (75) have investigated the effects of failure of a person in a group task upon others' perception of him. In groups of Ss where each S was dependent upon every other S for goal attainment, it was found that there was generally a greater change to a negative perception of the failing person than in groups of independent Ss. Exline (44) examined the sensitivity of Ss to the task- and ego-oriented behavior of other Ss in "congenial" and "uncongenial" groups. When Ss rated their own behavior as task- or ego-oriented, other Ss were significantly better at predicting others' task-oriented ratings in the congenial than in the uncongenial groups. There was no difference between the two kinds of groups in accuracy of predicting ego-oriented evaluations, although females were more accurate than males in doing so.

Harvey, Kelley & Shapiro (64) undertook an experiment dealing with change in characteristics of the judged person as a result of his behavior toward the judge. Each judge was evaluated by either a "friend" or a "stranger" who gave him one of four possible degrees of unfavorable rating. After the unfavorable rating, the judge perceived the raters as more negative on 15 items like "friendliness," "sense of humor," etc. Further, with a reversal or two, the amount of change to negative ratings increased as the unfavorableness of the rating the judge received increased.

Pepitone & Sherberg (114) were interested in the effects that perceived responsibility and intentionality would have upon the attractiveness of a punisher. The experimental conditions were set up by verbal statements of social situations describing a punishment situation where the punisher's intention was either "good" or "bad" and responsibility for the punishment originated in either the punisher or some external agent. Only the intention of the punisher affected attractiveness, which was lowered if his intention was "bad."

The "curious experiment" by Cofer & Dunn (31) referred to by Crutchfield (38) in his 1954 review is curious no longer. Kjeldergaard & Jenkins (85) have turned it every way but loose. With an exact repetition of the

Cofer & Dunn procedure of having Ss learn a word list containing either the word "warm" or the word "cold" and then rate personality traits of persons depicted in photographs, the results are, contrary to Cofer & Dunn, completely negative. An additional replication with a modified procedure produced negative results too. Of the hypotheses advanced by the authors to account for the conflict, the reviewer tends to favor the one which says "there is no effect to be studied."

In a traditional kind of impression formation experiment, Luft (97) reported that even adjustment has its price in our society. Ss predicted the personality questionnaire responses of a fictitious 30-year-old male American who was married and who had two children. One-half of them, however, were told he was making \$42.50 a week and the remainder \$250 a week. The college student Ss saw the "poor" man as being in serious adjustment difficulty compared to the "rich" man and also as compared to the actual scores of low income men.

Individual differences.—Stone, Gage & Leavitt (131) addressed themselves to the issue of whether an S can accurately predict another person's responses over and above the accuracy with which the stereotype of the class to which the person belongs predicts his responses. They first obtained the responses predicted for the stereotype and then the responses predicted for a member of the stereotyped group. The correlation between the numbers of accurately and inaccurately shifted items was negative. This correlation may mean that there were not simply shifting and nonshifting Ss but that there were at least accurately and inaccurately shifting Ss. Stone, Gage & Leavitt argued that this is not the case. They asserted that the judges were good at either accurate shifting or accurate nonshifting of responses from the stereotype. Discussing the outcome, the authors suggested that accurate shifters are most dependent upon the manifest stimulus value of the object plus the judge's readiness to depart from the stereotype. Unwillingness to depart from the stereotype plus either insensitivity to the stimulus value of the object or sensitivity to the object's real self must then characterize the accurate nonshifters.

Characteristics of the judge.—Getting at the more dynamic ways of producing differences in social perception, Feshbach & Singer (46) have published an excellent confirmation and extension of Murray's classic study (111) of the effect of fear upon the perceived characteristics of other people. Murray, of course, found that fear arousal produced complementary projection in the form of increased perception of maliciousness in the photographs of people. The Feshbach & Singer results indicated that when fear is aroused by electric shock Ss will see others' photographs as more fearful (supplementary projection) and possibly as more aggressive (complementary projection). They suggested that the solution to the differences between the two studies lies not in the fact the Murray measured only maliciousness, but in the relationships between the judge and the judged other person, with especial reference to the judge's expectations about the

behavior of the other person. Along the same general line of approach, Bosson & Maslow (21) reported what may also be supplementary projection. Their Ss rated 200 pictures of fellow college students along a "warm-cold" dimension. Students with high scores on a personal security measure judged their peers as "warm" more often than did the insecure Ss.

Scodel & Austrin (122), following earlier investigations, studied the effects of anti-Semitism (measured by the F scale) upon the perception of photographs of Jewish and non-Jewish people. They confirmed the usual result: highly anti-Semitic Ss label more persons as Jewish than those low in anti-Semitism, but the former are not more accurate. Jewish Ss also label more photographs as Jewish but are more accurate. The first finding is related to "passive acceptance of ethnic stereotypes by the high scoring group" and the second is explained on the basis of past experience in making such discriminations and perhaps a need to know on the part of the Jewish person.

Attitudes were also found to affect perceptions of group situations. Stone & Kamiya (132) dealt with the effect of one's own position upon perception of the opinion of others in the group and discovered that people generally overestimate the number of others sharing their own opinion. After group discussion of the topic, estimates shifted in the direction for which there was most expressed support, as did group opinion, with the upshot that the amount of error in the estimate of the consensus was not reduced. Overestimating the group's support for one's position has at least one limiting case, according to Ausubel & Schpoont (7). With highly structured group opinion—in this case religious attitudes—estimates of group opinion were not distorted by the Ss' own attitudes.

Perlmutter & Shapiro (116) looked at the effect which the context provided by one's own background has upon perceiving other people. Specifically, they ask how one perceives a person who makes extreme statements when this person comes either from one's own or a different national group. Because people better understand (have a larger number of more complex hypotheses about the reasons for?) specific acts occurring in the context of their own background, Perlmutter & Shapiro expected and found less stereotyped perceptions under that condition than where the backgrounds of the statement maker and judge were different.

Interaction between the characteristics of the judge and the judged object.—How do the personality differences of the judge and judged other person affect perception? Three studies were aimed at this problem directly and yielded remarkably similar results. Gordon (58) separated Ss into "repressors" and "sensitizers" by examining their scores on inventory scales of anxiety and defensiveness. A repressor was an S who had a high defensiveness score coupled with a low anxiety score, while sensitizers had the opposite pattern. The Ss were then arranged in assorted pairs to cooperate in producing a TAT story after which they predicted their partner's re-

sponses to the two scales. The results showed that repressors assumed for others greater similarity to themselves while sensitizers tended to predict that others would be different from themselves. Neither kind of S, however, was more accurate in predicting the others' responses. Repressors were more accurately predicted by both groups of Ss, but Gordon is quick to point out that all the Ss ascribed the positively socially valued responses to everyone, and that these are the responses which repressors give. It seems probable that to interpret this finding as one of greater accuracy would be to make the kind of mistake against which Crow & Hammond (37) have warned.

Leventhal (93) reported an experiment similar in kind to that of Gordon except his Ss were selected on the basis of cognitive complexity, using Kelly's Role Repertory Test followed by judged recorded interviews of cognitively simple and cognitively complex interviewees. Cognitively simple judges scored higher on assumed similarity than did complex ones, although neither type of judge was more accurate than the other nor was there a significant interaction between judge type and object type. When greater information about the object was provided there was increased accuracy on the part of both kinds of judges, with a tendency for the simple judges to be affected more than the complex type. It would seem possible to explain these data in terms of readiness to respond, too, if one assumes that simple Ss are set to perceive people—including themselves—as simply alike while complex Ss are set to perceive people as complex and different. When additional information is provided about the object, both types of judges improve, but in all likelihood the simple ones improve more because the added material disaffirms their "simple and alike" set more than it does the "complex and different" hypothesis of complex people.

Baker & Block (9), with a competent study, also failed to find any significant interactions between judge and object characteristics. The stimulus objects were nine sound films of stress interviews chosen so that the interviewees represented three levels of self-favoring behavior and three of ego control (over-, under-, and appropriate-control). The judges were also assessed on the basis of ego control. Self-favoring and overcontrolling objects were most accurately predicted. Does "set" come to mind? Appropriately, controlling judges show some slight tendency to be more accurate than under- and overcontrollers, who follow in that order.

Taken together the three experiments examined above give quite convincing proof that: (a) there are no significant interactions between types of judges and judged objects on accuracy measures and (b) the set of the judge seems to be a major determinant of social perception.

Consequences of social perception.—Two studies related accuracy of social perception to personal outcomes. In the first of these, Rodgers (118) related his employee Ss' role concepts to the employer's conception of the Ss' roles. Congruence between the two was found to be significantly related to job success. In another study, Stryker (134) put forward the hypothesis

that adjustment is a function of the accuracy with which an individual perceives the role of others with whom he is interacting. The opposite turned out to be correct insofar as adjustment of parents and the accuracy with which they perceive their children are concerned. Now, there's a finding with which you can really have unprofessional fun! Stryker went on, however, to show that a number of contingency variables affected the outcome: e.g., traditionality of the parents. [Helper (66), by the way, reported that parents are not very accurate at perceiving their children's self-evaluations; nor are they accurate in perceiving their children's social acceptability.]

Fiedler (49) again cited evidence to show that groups whose leaders perceive the least and most preferred co-workers in the group as similar have less effective work groups than leaders who perceive a greater difference between such co-workers.

Taylor (137) has looked at how Ss display their feelings in groups to the extent that others may become aware of these feelings. Typically, Ss do display their feelings sufficiently so they can be perceived at better than a chance level, and accuracy increases with an increase in length of acquaintanceship. Of particular interest at this point, though, is the fact that unpopular group members are reserved in displaying emotion and are ignorant of the emotion they arouse in others.

AUTHORITARIANISM AND ACQUIESCENCE

It has been almost ten years since Adorno *et al.* (1) published *The Authoritarian Personality*. After such a length of time, most publications in psychology have lost much of their immediate impact on research but not *The Authoritarian Personality*. Already cited above are several studies making use of the scales and concepts of the volume, and the research now to be considered is almost completely derivative from it.

Bass (10), of course, is responsible for a considerable amount of the year's work because he reopened the controversy over what the F scale measured. He showed that when the content of the F scale's original items was reversed so that disagreement with them was required in order to score in the authoritarian direction, a correlation of $-.20$ was obtained between the original (F+) and reversed (F-) items. Bass interpreted this and factor analytic results to mean that the F+ scale was actually measuring "acquiescence," a readiness to agree with any presented statement.

An interesting exchange over this original finding took place this year. Messick & Jackson (105) found errors in Bass' statistical procedures but agreed that Bass' hypothesis is probably right on the basis of other evidence. Bass (11) admitted that he was right for the wrong reason, and finally Kerlinger (81) came along to demonstrate that by playing games with the factor solutions one can prove that the F+ scale measures either acquiescence or authoritarianism. His last sentence says, "The analysis of one coefficient of correlation can get very complicated," which wins the reviewer's vote for the understatement of the year.

As to new data, no less than eight studies gave evidence relating the F+ to acquiescence, and all of them pointed to acquiescence being a major component of whatever it is that F+ measures [Beloff (12), Carey, Rogow & Farrell (25), Chapman & Campbell (27, 28), Christie, Havel & Seidenberg (29), Gage, Leavitt & Stone (54), Jackson & Messick (73), and Jackson, Messick & Solley (74)]. Of these articles, three (12, 25, 54) were especially significant in that they showed F+ to be related to measures of acquiescence other than F-.

All the papers taken together, however, have not presented convincing evidence that a response set to acquiesce can account for even most of the variance in F+ scores. This is the point which Christie, Havel & Seidenberg (29) took up in a paper which should be read by everyone with even a passing interest in the problem. After first analyzing previous attempts to construct F- scales and concluding that no one of them had really come up with the required opposite psychological meaning, they painstakingly constructed an F- scale of their own. The authors then administered F+ and their F- to a number of groups of Ss. Most of the groups were undergraduate college students, but there was a group of Washington lobbyists and another of graduate students. The correlations between authoritarian scores on the two scales ranged from .10 to .58 with a median of about .41. One could have hoped for higher r 's but only the lowest r was not significantly different from zero. In discussing the results, Christie, Havel & Seidenberg pointed out that only some of their Ss were set to acquiesce and, further, that some low scorers on F+ had a set to reject, although this tendency did not occur often. Additionally, the evidence showed that acquiescence is highest among new college students. This was interpreted by the authors as the result of a conflict between college and home values, which led those Ss really to accept both F+ and F- items. Whatever the final outcome of the acquiescence vs. authoritarianism issue, this article will have contributed a great deal towards solving the problem.

Relationship of authoritarianism to other variables.—As usual, authoritarianism has been related to a number of other variables. Gregory (59) found a positive correlation of .53 between F+ and religious orthodoxy (literalistic and materialistic religious orientation); Mowry (110) reported a significantly negative correlation between F+ and a test of ability to deal with supervisory problems; and Hart (63) presented data on the way authoritarian parents (partly determined from F+) chose to punish children's misbehaviors. The more authoritarian the parent, the more likely is he to use a punishment involving bodily harm ($r = .63$). Rosenberg & Zimet (119) stated that high scoring authoritarians have a preference for simple, symmetrical, and two-dimensional—rather than three-dimensional—figures. Milon (106) repeated and confirmed the study by Block & Block (18) in which it was found that high scorers on F+ stabilized their judgments of the amount of movement in the autokinetic illusion faster than did low scorers. Milon went beyond that point, however, and gave evidence supporting the

added conclusion that high scorers are also less likely than low scorers to change their judgments when an attempt is made to break up the norm by introducing a second autokinetic stimulus.

All of the above studies seem reasonable in their fit to the proposed personal qualities associated with authoritarianism. There are some negative results, however, along with several words of warning. To cite a few, Hardy (62) found no significant relationships between F+ and the major variables in his study of conformity and attitude change. Perlmutter (115) analyzed the content of F+ and showed it to be inadequate to measure the authority orientation presumed to underlie xenophilia. Livson & Nichols (96), with data to back them up, warn that any of the California scales E, F or PEC [Political, Economic Conservatism] may have quite different statistical characteristics when used with populations for which the scales have not been tested for appropriateness. Cohn (34) had another warning in regard to the F+ scale. His data showed that F+ scores can readily be affected by the behavior of the administrator and that the characteristics of the Ss affect the amount of change: Ss with high verbal aptitude were more likely to change their F+ responses than Ss with low verbal aptitude. Also, there was a caution of another type in Libo's (94) results: relating authoritarianism to attitudes and opinions may yield either positive or negative findings depending upon other contingency variables such as saliency of the attitude or opinion to S's ideology.

SOCIAL INTERACTIONS AND THE GROUP PROCESS

Communication networks.—This area, which seems one of considerable importance, received little attention last year. The most important result with formal patterns was obtained in the experiments of Guetzkow & Dill (60) who worked with five-man patterns solving the simple common symbol problems originated by Leavitt (92). By a set of clever experimental procedures, Guetzkow & Dill were able to show that, in structures which severely limit the communication channels available to the group, the organization of work which takes place is accomplished in terms of each individual learning to use his own position to the best advantage of the group. In groups with more communication linkages among their members, on the other hand, organization, if it takes place at all, must be induced by insightful planning by the group. The authors emphasized another point which has long needed underlining: networks with a limited number of channels may prevent the selection of the optimal person for a particular position in the problem-solving organization.

Shaw (124) and Shaw, Rothschild & Strickland (125) carried forward Shaw's approach to formal communication patterns. Both reports confirmed earlier findings to the effect that, when fairly complex problems must be solved, highly centralized structures such as "star" (Leavitt's "wheel") are slower than networks of a less centralized type (e.g., "comcon," a com-

pletely connected structure where each S can communicate with every other S). The data presented by Shaw *et al.* (125) suggested that the difference might be caused by persons in a central position attempting to obtain compromise solutions when the group is not in agreement on the correct answer. In the other study, the presence of information irrelevant to the problem was shown to have more effect upon star structures than it did in less centralized ones.

In a continuation of a series of studies, Lanzetta & Roby (89) performed an experiment using completely connected three-man groups. Their interest was primarily in how certain work load conditions and dependence upon other group members for information affect the process of group learning. The results are complex and not easily summarized, nor is the relation of the findings to other network studies clear. One conclusion, however, stands out: "Apparently, left to themselves under constant situational pressures, groups modify their behavior at a characteristic pace which is independent of the demands placed on them."

Trow (139) attempted another answer to the problem of what underlies the usual finding that a person in a central position in a network rates his job-satisfaction higher than does a person in a peripheral position. His data indicate that the degree of autonomy one has in job performance is critical to job-satisfaction and may override that job-satisfaction deriving from centrality.

Outside the laboratory, Kipnis (84) examined how sociometric preferences in aircraft crews were related to the communication structure required to operate the aircraft. She found a clear-cut determination of such preferences by the organizational pattern. The amount of contact required by the group organization facilitated the development of stronger preferences for the individual contacted, which supports the laboratory result for sociometric choices in communication networks.

Coleman, Katz & Menzel (35) and Larsen & Hill (90) again demonstrated how sociometric linkages determine communication flow. Coleman *et al.* determined the sociometric pattern which existed within a group of 125 physicians. The month during which each physician used a new drug was obtained from his prescription record at a later date. Doctors chosen more frequently by their colleagues were more likely to have early dates for first use of the drug. Additional evidence was presented by Larsen & Hill who observed communication in a boys' summer camp.

Before leaving this issue it should be mentioned that Winthrop (143) has presented a mathematical theory of behavior and information diffusion in groups. In two examples, he obtained a very close fit between expected and observed distributions.

Composition of the group.—Composing groups of selected Ss seems to be another neglected area of research. Most of the time Ss are gathered willy-nilly, brought into the laboratory, and told to interact about something

[almost anything, a fact Roby & Lanzetta (117) rightly deplore]. If one subscribes to the belief that there is no such thing as THE group, our usual way of making up groups in the laboratory is often ridiculous and may account for a large number of the conflicting results as well as the all too frequent statement "the hypothesis was sustained ($p < .08$, one tail)."

Cleveland & Fisher (30), although they didn't intend to, furnish a good example of how variability may be increased by lack of attention to the personality characteristics of persons who make up an interacting group. They related the kinds of activities and discussion content in groups made up wholly of Ss having either firm or vague boundaries for their body image schema. In the firm boundary groups of Ss, there was greater concern with achievement and individuality than in groups having Ss with vague boundaries. No one, of course, would advocate matching groups on this or any other one measure or set of measures. Certainly, however, the characteristics of Ss selected for group experimentation should receive more thought than they currently do.

Cervin's (26) experiment could also be cited as a case in point but it is important in a number of other respects. As part of a larger attempt to apply general behavior theory to social interaction patterns, Cervin utilizes certain constructs of learning theory to characterize his Ss and then predicts how they should behave in social settings in addition to predicting the outcome of the interaction between two persons of different characteristics. Specifically, he predicts "that the individual with the higher reaction potential should be in the ascendant position on all three measures of behavior" (participation in the discussion, speed of first statement, and resistance to change of opinion). In dyadic groups of Ss, the outcome was as predicted, but even if it hadn't been one could still wish that more work in social psychology were as closely reasoned and as thoughtful as this piece.

An additional two studies can be classified under the heading of group composition: Smith (126) and Berkowitz, Levy & Harvey (15). In the former study, confederates of Smith remained silent in problem-solving discussion groups, either with or without an explanation by them that they usually were passive in group situations. With silent members, the other group members were less productive and satisfied with the group situation and more defensive than in the absence of such members. When the silent ones clarified the reasons for their silence, increased productivity and satisfaction resulted.

The investigation by Berkowitz *et al.* labelled each of three members of a group as either a success or a failure in performing his part of the group task. The results confirmed Gilchrist's (56) finding that persons are positively oriented towards successful other persons, but showed that such orientation occurs only in strongly task-motivated Ss.

Effects of the group upon its members.—The members of groups in which each member is dependent upon others for goal attainment feel the

impact of the group situation more than groups whose members are independent of each other. Berkowitz (14), for example, found the members of dependent groups highly motivated toward the group task when compared to their independent counterparts. Thomas (138) has shown that group members feel more responsible, produce more, and report a greater amount of emotionality in dependent than in independent groups. He also found that whether one's own or the group's performance is to be evaluated made a difference: if the group is to be evaluated, S feels less responsible and is more likely to vote that the group stop for a rest than if S himself is to be evaluated. The group, then, has both threatening and protecting properties according to the condition under which it operates.

Lichtenberg (95) has given another way in which the group provides for self defense against expectation of failure in a task. He tested the position that failure in the first stages of a cooperative task would reduce estimates of the probability of successfully completing the task less than such a failure would when a person worked independently. The hypothesis was sustained. Lichtenberg accounted for the result by saying that a cooperative situation allows failure to be perceived as the result of faulty coordination among Ss, a factor which the Ss see as correctible.

DeCharms (40) goes a step farther in qualifying any broad statement about the impact of a group upon the individual. He argued that a person high in threat-oriented need for affiliation should produce differently in competitive as against cooperative groups after being threatened with rejection. He found this to be the case in general and attributed the result to hostility in the experimental Ss resulting from the threat of rejection.

Other factors involved in how the group affected its members are given by Stotland *et al.* (133), who looked at the members' self evaluations after personal success or failure in group activity. A successful S was not affected by any other factor; but with a failing S it was a different story. Under the circumstance of failure a relevant task caused a drop in self-evaluation, particularly when the group had high expectation for its own performance. The authors believed that most of the effects could be understood in terms of the failing person not knowing what response the group will have to the failure of one of its members.

Gerard (55) and Wispé & Thayer (144) both dealt with the consequences the clarity of one's role in a group has for an individual. Gerard, using laboratory conditions, found his Ss to have considerable concern about their performance and a low opinion of their effectiveness when their roles in the group were unclear. Wispé & Thayer found an industrial situation where a set of employees intermediate in the status hierarchy had ambiguous role definitions. This ambiguity gave rise to some anxiety as indicated by the men's interview responses.

In concluding this section, an experiment by Pepitone & Kleiner (113) should be mentioned. They were interested in the factors which change

group cohesiveness. Following a need-satisfaction paradigm, they predicted that where status differentials exist between two groups, cohesiveness of Ss in the high status group would increase as the probability of loss of status decreases, and that cohesiveness would increase in the low status group as the probability of a gain in status increased. The first of these predictions was supported by the data but the other was not. The predictive failure was explained as being due to (a) projection of failure by the low status, low probability-of-gain Ss and (b) their withdrawal from between-to within-group oriented behaviors.

Along the same lines, Cohen (32) reported a replication of Kelley's (78) well-known study of communication in experimentally created hierarchies with an additional gimmick: not only was there a straight status differential between two groups working on the task, but the high Ss were empowered to evaluate the behavior of their low-status colleagues. There was, of course, the second factor of mobility of the groups. Results for the high groups confirmed Kelley's original finding, but the addition of power to high status clarified the earlier result for the low-status Ss: Ss in the low-mobile group sent more and longer messages to the high-status level while Ss in the low-nonmobile group sent more and longer messages within their own status level. The content of both sets of messages indicated that communication was oriented around obtaining some sort of need satisfaction.

Leadership.—Interest in the problem of leadership style continues. Solem (127) and Fox (52) examined the effects of permissive, democratic, and delegating leader behavior as opposed to the restrictive, authoritarian, non-delegating type of behavior in problem-solving discussion groups. Both studies reported that more participants were satisfied with the permissive, etc., leader, although Fox noted that discussions were twice as long with that style.

Ziller's (146) experiment with aircrews and their commanders points to essentially the same conclusion, although the time allowed to reach a decision was limited in this case. Four procedures were used. In the first one, the commander simply announced the procedure the group would use in reaching a rescue rendezvous in a situation involving risk to the crew. In the second procedure, the commander announced the decision but asked for the crew's ideas. Under the third method everyone discussed the situation both before and after the commander gave his own position, and in the fourth the crew itself made the decision. No differences were found among the conditions as to satisfaction with the decision or agreement with it but there was dissatisfaction with the way it was reached in the simple announcement condition. As an aside, it is interesting to note that more crew involvement in the process produced more decisions in favor of a course involving greater personal risk for the crew.

Maier & Maier (100) took a different tack in approaching the style prob-

lem. In the first of two techniques, the leader was permissive, made no value judgments himself, and exercised no control over the direction of the discussion. The second procedure also allowed the leader to behave permissively but required him to attempt to get agreement on a set of sub-problems before the group made its final decision. The results show that the second procedure was a little more likely to produce a "good" decision.

The effect of subsequent events upon emergent leadership in task-oriented discussion groups received attention from Katz *et al.* (77) and Borg (19). The first of these looked at what the external imposition of discussion tasks did to the status of emergent leaders. Allowing the three- or four-person groups to discuss one problem and then to choose another for continued discussion, the experimenters either allowed a group to discuss its chosen second problem or imposed a different one. The results showed: (a) leaders who emerged over considerable opposition are likely to have lost their status by a later time, especially if an external agent imposed a task on the group; (b) leaders having little initial opposition were stable unless the second task was imposed (two-thirds of them were deposed if it was imposed); and (c) leaders with little initial opposition, but who had their choice for the second topic rejected by the group, were stable in their position if the second task was imposed. This last finding apparently occurred because the weak opposition to the leader was undermined by the act of task imposition.

In the other study concerned with the problem of emergent leadership, Borg (19) was interested in what happened to an emergent leader when another and formal leader was appointed by an outside agent. The data showed that, even though the emergent leader still evidenced more leadership behavior than the other group members, his effectiveness was reduced and that this reduction was not compensated for by the effectiveness of the appointed leader.

Other studies of group processes.—Another of the areas which should receive more attention from social psychologists is the experimental analysis of groups in crisis situations. Maybe the spur will come from the issue of *Human Organization* edited by Demerath & Wallace (40a), which is wholly devoted to the problem. It contains, incidentally, a good annotated bibliography. One experimental study, by Hamblin (60a), appeared on the topic. In this study, groups of Ss played a fast-moving cooperative game whose rules suddenly changed without warning. Hamblin had expected that group integration would increase in the face of this change. It didn't. A number of measures revealed that the opposite had occurred. The reason for this result would appear to lie in the nature of the task itself and in its presentation to the Ss.

Ziller (147) described the effect of group size upon the quality of group decisions. His groups were composed of from two to six persons who worked together on a sequence of two decision-making tasks. The result was

that the larger the group the more accurate was its decision. The relation was not linear and one wonders what happens when the number gets much larger.

Cohen (33a) adds another bit of evidence to the effect that judgments of social stimuli are basically no different than judgments about the heaviness of weights or the brightness of light sources. After allowing pairs of Ss to judge a series of descriptions of behavior and to judge each as to its social desirability, Cohen introduced other statements from a different range of desirability, and observed the changes which took place in the judgments. The Ss, judging together, had established a norm before the new range was introduced and its introduction shifted the norm appropriately. The results duplicated, with social stimuli, those one would expect from any straightforward psychophysical experiment of the same design.

CROSS-CULTURAL STUDIES

Several years ago, the reviewer made the mistake of telling an anthropologist friend that he thought an anthropologist's real job should be that of comparative social psychologist. This was a serious mistake for it was followed by several discourses on the importance of basket weaving before any warmth returned to the relationship. Since then, the reviewer's publicly expressed opinions on the matter have conformed to the norm of his anthropological colleagues. It is a pleasure to report, however, that it begins to look as if the open job title is going to be taken over by psychologists. There have, of course, been a few—mighty few—psychologists who, like Dennis, have been investigating across cultural lines for years. Recently, the number has been increasing, and it should. In the hope that it will, the following studies, although most of them would fit elsewhere in this review, have been placed together to highlight their presence in the literature.

In one of the most interesting of the group of studies, Allport & Pettigrew (3) tested the occurrence of the trapezoidal window illusion among Zulu Ss of different degrees of acculturation. The unacculturated Zulu group is one in which the frequency of circular forms definitely predominates over rectangular ones, which rarely occur. It provides, then, an excellent setting for checking the effect of experiential background upon the appearance of an illusion dependent upon rectangularity. Allport & Pettigrew, in a neat series of experiments, were able to demonstrate that under optimal viewing conditions the illusion occurs equally often in acculturated and unacculturated Zulu groups, and in a European group. When viewed suboptimally, however, acculturated Ss report the illusion more frequently than do unacculturated Ss. The authors conclude that "object connotation (meaning) based on relevant cultural experience helps to determine the nature of perceived movement under marginal conditions."

Bagby (8) also studied perception in different cultural settings. He pre-

sented his American and Mexican Ss with mismatched pairs of stereogram slides, one of each pair being related to one of the two societies from which his Ss came. In this situation of binocular rivalry, that half of the slide related to the individual's cultural background was most frequently reported as being seen.

In another African study, Doob (42) analyzed the effects of contact with Western European culture in members of three societies, the Ganda, Luo, and Zulu. He set up a series of hypotheses regarding antagonism toward and deviation from traditional authorities and practices, hostility towards Europeans, etc. An attempt was made to sample the populations in which he was interested ("... sampling in Africa is not easy," Doob says, and from his description of the situation he can hardly be accused of overstatement). Two hour interviews were obtained using a variety of techniques with as many of the sample as possible. The important thing about this report, though, is not that Doob's hypotheses were sustained, as they mostly were, but that, in discussing his results and problems, Doob offered sound suggestions for improving psychological research in the area of acculturation.

On the general matter of norm conflict which is basic to the acculturation problem, French & Zajonc (53) presented a closely reasoned theoretical analysis of norm conflict across culture lines and proceeded to test it, using Indian students as Ss. The experimenters' generally Lewinian theoretical position treated norms as force fields, and, thus, norm conflict as a situation of overlapping forces. For intragroup norm conflict, knowing the relative strengths of the forces is sufficient to predict the behavior which will occur. Where the norms arise from two different groups, as is the case with cross cultural conflict, situational potency, defined as the degree to which a given group membership is aroused relative to others, becomes important. For predictive purposes, then, it was necessary to control and manipulate situational potency since it was assumed to have the effect of increasing the force of the norm of the situationally more potent group. Choosing behavior whose norm differs in India and the United States (showing respect for professors), they performed a well-conceived experiment which included a bold attempt to predict the magnitude of each S's response. The observed pattern of behavior was consistent with theoretical expectations, but statistically significant differences were lacking in a number of instances.

Dennis (41) and Hamilton (61) both dealt with language behavior in several cultural groups. Dennis' procedure, which he suggested would be useful in many exploratory cultural studies, was to ask children the question "What is _____ for?" where the blank was filled in by the names of common objects such as "mouth," "cats," and "sand." Working with American, Lebanese, and Sudanese children, Dennis found that their responses to the question demonstrated that children of different cultures differ considerably in their concepts of common objects as early as five years of age. Ad-

ditionally, from the children's responses it was possible to isolate consistencies in the interests, values, and activities which are being developed in different groups of children.

Hamilton's contribution also consisted of a method for utilizing language behavior in interpreting the behavior of culture groups and individuals. The procedure, used with Hopi-, Dakota-, and English-speaking Ss, consisted of placing 27 nonsense objects before an S and asking him to talk about them. The samples of language were then scored in terms of six major ascriptive categories. Various comparisons among these and other measures indicated a number of differences among groups which, Hamilton suggested, meant real differences in the way the individuals dealt with their environment, the concern they felt for self, etc. If so, the method may have real possibilities, but it will need considerable validation.

Mischel (107) working with children of either Negro or Indian parentage in Trinidad attempted to confirm some suggestions from the ethnological literature about the two groups. The ethnological data pointed to the Negroes as being impulsive and self-indulgent, having a short time-perspective, and preferring an immediate small reward to a later large one. The Indians were said to display characteristics of an opposite kind, particularly on the last factor. Using seven to nine year old children from the two culture groups, Mischel recorded their choices when they could elect to receive either a penny candy immediately or a ten cent candy a week following the choice. A significantly higher number of Negro than Indian children chose the immediate small reward.

Mischel found another variable even more closely related to the choice, however: the presence or absence of the child's father in the home. When this factor was controlled, the cultural differences disappeared. The child was more likely to choose the immediate reward when the father was absent from the home, a fact which should spread joy among our psychoanalytic colleagues.

McClelland *et al.* (102) and Bruner & Perlmutter (23) present data derived from Western European and American Ss. The first group of investigators were interested in the different personally-felt obligations to self and society in the United States and Germany. The results showed that the major obligation to self was achievement-oriented in the United States, whereas in Germany it was to become a separate, unique entity. Societal obligations in the United States were directed towards group participation, while in Germany they were organized about duty to an "impersonal other."

In an excellent example of how cultural and national groups may be made to serve experimental purposes, Bruner & Perlmutter used subjects from three countries, France, Germany, and the United States, in a test of two hypotheses regarding impression formation. They reasoned that (a) the categories in which a given person is placed should have more effect when experience with members of the category is low rather than high and

(b) the categories are more effective when one must view an array of persons distinguished by their categories than when the persons in the array are not so distinguished. They then assigned nationality characteristics to written descriptions of people and made use of the nationalities of the Ss to test the hypotheses, which were strongly supported by the data.

A CONCLUDING NOTE

It was difficult to read last year's output in social psychology without sometimes agreeing with Borgatta & Cottrell (20), who take research in the area to task for being unsystematic and for rarely dealing with anything more than verbal behavior. At times, as a matter of fact, it was difficult not to feel like agreeing with a pair of Russians' (144a) assessment of social psychological research: "(Some) reports presented to the (Fourteenth International) Congress . . . had no scientific significance whatever. Among these were, first, many papers dealing with the so-called social psychology." These negative reactions were infrequent, however, compared to the number of positive ones which were occasioned by studies (a) testing precisely stated theoretical positions, (b) using many levels of a dimensionalized independent variable rather than a dichotomized one, (c) measuring observable behavior in social interaction situations rather than inferring it from questionnaire data, or (d) connecting social psychological phenomena to other psychological theory and knowledge. If studies of this type continue to increase in number, there is good reason soon to expect really significant progress in understanding and predicting social behavior.

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ABNORMALITIES OF BEHAVIOR¹

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This is the tenth year in which the literature on abnormalities of behavior has been surveyed in the *Annual Review of Psychology*. A backward glance over the decade shows that there have been certain significant changes of interest. The shadows of war still lay over the earlier volumes, which gave considerable attention to war stress and to such war-born investigations as the Minnesota semistarvation project. Even more popular at that time were the lobotomies and the convulsive therapies, reports of which were occupying large amounts of space in psychiatric journals. Last year, in contrast, there were relatively few papers on these forms of treatment. Those on lobotomy and on insulin shock had decidedly the air of a mopping-up task, whereas those on electroconvulsive therapy might be described as sedately mature and judicious. But youthful vigor and unguarded optimism are still abundantly present: one finds them now in the flood of articles on the wonderful effects of the ataractic drugs. Some of us who accommodated ourselves with difficulty to the idea of treating behavior disorders by violence are much gratified that the fashion has swung toward treating them by chemically induced tranquility. Yet it is disquieting to find in sober science such rapid changes of enthusiasm as have characterized recent work on the physical side of mental disorders.

To understand any topic of research it is helpful to consider where it stands in what may be called the cycle of discovery. The first stage, which we may call birth, is ushered in by some kind of break-through, some momentous discovery of fact or technique which opens a whole new range of possibilities. Then follows a period of childhood, buoyant in spirit, during which the discovery is exploited and extended. Reported results are mainly positive, a simple systematic explanation seems to be forming, and therapeutic effects, if they are involved, are said to be astonishingly good. Next comes the third or adolescent period, filled with doubt and criticism; reported results are apt to be inconclusive or negative, rival theories become prominent, and therapeutic effects are dubiously satisfactory. The fourth stage is one of maturity, marked by the appearance of sophisticated discussions in which the limits of the original discovery are recognized and its precise consequences worked out in detail. Then comes senescence, when research settles into a plodding routine or concerns itself with minute and rather precious details, and when theory becomes conservative and orthodox. The urge toward discovery is so insistent that we rarely witness a final stage comparable to death, though the spectacle of a research area in bad

¹ This review surveys the literature from May, 1957, through April, 1958.

need of a new break-through is familiar to all of us. Usually there is somewhere a growing edge and a hopeful progress toward rebirth.

This statement of the cycle of discovery is surely too simple and perhaps too whimsical, but it may prove to be a useful device in understanding the year's events in the several areas now to be considered.

NEUROSES

The study of neurosis is in a late stage of the cycle. Few papers are to be found on the forms of neurotic disorder which once played such a dramatic part in establishing the psychogenic point of view. The hysterias and obsessional neuroses upon which Charcot, Janet, Breuer, and Freud made their momentous observations are no longer serving as focal points for reflection and research. Even in the psychoanalytic literature interest has migrated to schizophrenia because of the light it may shed on ego psychology and the earliest stages of development. Taking the literature on behavior disorders as a whole, schizophrenia is by all odds the major theme, as it is the major social problem; the proportion of papers in the journals is rather similar to the proportion of mental hospital beds occupied by schizophrenic patients.

There is a widespread belief that the decline of interest in the neuroses comes partly from the fact that such disorders are no longer commonly encountered in the clinic and consulting room. Wheelis (79), among others, is impressed by this change, seeing in it evidence for the decline of those values which produced the strict nineteenth century superego, and their replacement by parental permissiveness, adolescent other-directedness, and general difficulties, sometimes of clinical proportions, in establishing a sense of identity. Bernstein (4) also reflects upon this historical change, which he is able to attach to a fairly definite date through the observation that just in the last few years college students have begun to look with surprise upon case histories involving strict superego-producing households and to declare that it was never like this in their own families. Be this as it may, one has the impression that neuroses are no longer considered a likely source of fresh light on abnormalities of behavior. Those who write about them are chiefly concerned with rather precious details in long-term psychotherapy.

Yet we cannot conclude that the problems have been solved. In a lively paper Chodoff & Lyons (8), following earlier leads of Kretschmer and Bowlby, question the old assumed relationship between hysterical conversion and the hysterical personality. Having found 17 patients with clear conversion symptoms—showing that the pattern is not quite extinct—they point out that only three really fit the category of hysterical personality type as this has been so often described. Conversion thus emerges as a special mechanism not firmly linked to a pattern of personality. Concerning the

hysterical personality pattern the authors make the interesting suggestion that it sounds like an exaggerated description of femininity as conceived by male psychiatrists. Perhaps the classical work on the grand hysterics is due for some critical reappraisal.

The most extensive work of the year on neurosis, however, is one which takes a wholly negative view of the psychogenic cycle of discovery. Eysenck (16) places his book in an entirely different historical tradition. Freudian psychoanalysis, he tells us,

has by now sufficiently demonstrated its fundamental failure to generate testable hypotheses, to make possible modifications in human conduct, and to account for the results of experimental work; it therefore appears desirable to return to another approach, which is historically linked with the name of Pavlov.

Some readers may feel that this statement sets the atmosphere poorly for the rigor which Eysenck claims for his work, but much interest will be aroused by his attempt to carry conditioning and learning theory as far as they will go in explaining neurosis. Account must be taken of two very general factors which contribute to neurosis: the neuroticism factor and the factor of extraversion-introversion isolated by Eysenck in his earlier work. Hysteria and psychopathic personality are extraverted neuroses, whereas anxieties, reactive depressions, and obsessive disorders are introverted. Patients can be sorted into these two categories by means of test batteries and can then be used in laboratory experiments designed to detect fundamental differences.

The hypothesis is then advanced, derived from Pavlov, that in extraverted neurotics excitatory processes will be slow and weak, inhibitory ones quick, strong and lasting; conversely, in introverted neurotics, excitation will be quick and strong, inhibition slow, weak and transient. A variety of experiments involving such items as speed of conditioning, figural after-effects, and satiation yield differences between the two groups which are small but usually significant and in the expected direction. Conditioning is fast and strong in the group of anxious, depressed, and obsessive patients, slow and weak in hysterics and psychopaths. Since social training consists largely of conditioned anxiety reactions, it follows that the first group becomes overtrained, with impulses overcontrolled, whereas the extraverted neurotics are undertrained and tend to act out their impulses. Eysenck makes the prediction that depressant drugs, which presumably increase cortical inhibition and reduce excitation, will make behavior more extraverted and that stimulating drugs will make it more introverted. He cites experiments, new and old, which generally bear out the prediction.

Readers with an historical bent will notice a marked similarity between Eysenck's work and that of Janet, who similarly divided neurotics into two broad classes and emphasized innate peculiarities of nervous functioning. Will history deal more kindly with this new attempt, which has the advantage

of close linkage with current learning theory? Many workers will find difficulties with the wholesale lumping together of patients actually so very different, and with the use of such scant and generalized variables; but if Eysenck's methods point the way toward basic constitutional factors in neurosis and the choice of neurosis, they deserve the most careful study. Treatment would presumably not be quite the same if its goal were seen as the best pattern of life permitted by each patient's constitution rather than as a universal pattern of mental health. Eysenck, however, views treatment in a much simpler light: it is to be conducted by laboratory methods of deconditioning in order to extinguish the stimulus-response connections which are responsible for symptoms. In this view he is abetted by a Maudsley fellow worker, Jones (35), who works out various methods of treatment based on conditioned reflex theory, and by another colleague, Franks (20), who reviews the literature on alcoholism and brings this disorder into the tradition of Pavlov. Obviously these ideas are not destined for a quick victory among people who treat patients, and it is to be hoped that they will not obscure the more solid part of Eysenck's contribution.

Returning to a more psychodynamic tradition, two publications by Dixon, de Monchaux & Sandler (10, 66) at the Tavistock Clinic describe an attempt to analyze the patterns of anxiety in neurotic patients. Choosing from the Tavistock Self-Assessment Inventory 26 items classified as "social anxiety," and using Thurstone's centroid method of factor analysis, these workers discovered a large general factor and four group factors: timidity about social initiative and assertive behavior, anxiety focussed on the control of bodily functions, fear of exhibition and being seen, and fear of revealing inferiority, this latter form often going with aggressive feelings toward others and ideas of reference. In contrast to their neighbors at the Maudsley, these workers are bent on discriminating among different foci of anxiety, with consequent refinement in the description of disorders which in the most general terms might seem very much alike.

Along one edge of the otherwise well-tilled field of the neuroses there is a development which has some of the freshness of the second or childhood stage in the cycle of discovery. This is in the area of school phobias and learning difficulties at school. Sperry *et al.* (73) attribute learning difficulties to a family pattern in which renunciation and denial are prominent, and in which the patient has been particularly eligible for the role of the unsuccessful one. Waldfogel, Coolidge & Hahn (77) describe the parents of children with school phobia as loving and devoted but badly entangled in a symbiotic dependent relationship with the child.

This theme is very lucidly developed by Eisenberg (14), who points out that in school phobia it is possible to witness the precipitation of a neurosis practically as it occurs, not as it is reconstructed from memory. The mothers, needing for their own reasons the child's continued dependence, clearly

communicate their anxiety at separation even while trying to offer reassurance that all will be well. According to Eisenberg, the mothers are more anxious than the children, who may have developed a certain unwitting skill in manipulating their frightened protectors, and it is for this reason that early school attendance can be required as a first step toward treatment—mainly of the mothers. One fears that unwary clinicians may start to caricature the “schoolphobicogenic” mother and use her as a convenient scapegoat. Perhaps the poor women have become intimidated about separation through reading books about psychiatry. But it is a great virtue of Eisenberg’s paper that he undertakes to observe directly the interaction of mother and child and the communication that is taking place between them.

PSYCHOSOMATIC DISORDERS

A curious fate has been in store for psychosomatic disorders in the cycle of discovery. The initial break-through came some 25 years ago with the work of Dunbar and Alexander. The psychosomatic idea, of course, is ancient; what gave this work the character of a break-through was not merely the general proposition that emotional problems create organic disorders but the more startling suggestion that specific patterns of emotional disorders produce particular kinds of organic disease. It was the vision of accident-prone personalities, ulcer types, colitis people, hypertensive characters, and other such specific relations, together with the hope that psychotherapy might triumph in a new field, that carried psychosomatic medicine through its first years of enthusiasm.

The adolescent reaction of criticism and doubt, however, proved to be unusually severe. The specificity hypothesis was difficult to sustain. Emphasis shifted to general stress, to constitutional susceptibility, and to ideas such as Grinker’s about the early conditioning of individually different patterns of autonomic response to stress. With this broadening, the psychosomatic idea spread out to include a much wider range of organic disorders such as tuberculosis and cancer, so that the field lost the sharp individuality to which at first it seemed destined. It has become a vital and valuable point of view in medicine as a whole.

Some of the year’s outstanding publications seem to be pursuing the earlier goal of specificity. This is true of Garma’s (25) monograph on peptic ulcer, in which a specific precipitating situation is claimed (being in love with someone who does not satisfy genitally and having an exacting occupation which allows little discharge for emotional tension) together with a specific infantile constellation (a bad-internalized mother who acts aggressively). It is somewhat true of a paper by Meyer & Weinroth (52), who try to locate the emotional root of *anorexia nervosa* and who make the interesting suggestion that the problem is pre-Oedipal and essentially psychotic rather than neurotic. Ring (63) comes forth as the champion of

specificity in a report describing the prediction of the form of organic ailment from interviews in which the ailment was not mentioned. Giving himself three choices with each of 134 patients, he made 88 correct first choices and only 12 complete misses. This experiment appears to have been well controlled: many more than 134 subjects were rejected because they hinted at their disease, and all subjects were so placed and draped that only their faces could be seen, to reduce possible constitutional cues. We must conclude from Ring's work that the specificity hypothesis is by no means dead.

Still inclined toward specificity is a report by Long *et al.* (48) on asthmatic children. Unfortunately the emotional constellation which they specify, involving the child's wish for closeness and "the mother's wish to maintain the child in an infantile dependent state," is precisely the constellation already mentioned as characteristic of school phobia, and the case for specificity is thus somewhat weakened. Knapp & Nemetz (41), also dealing with asthmatics, report that while personality disturbances are the rule there is no single type of disturbance that is characteristic of all their patients. Badal, Driscoll & Maultsby (2), studying ulcer patients who had been successfully treated medically or surgically, show that in 21 out of the 30 cases some other disorder, sometimes of a clearly psychosomatic nature, comes in to take the place, so to speak, of the condition that has been cured.

Moving more in a constitutional direction is a research study by Southworth (72), who investigated the possibility that muscular tension might be the preferred response to stress in patients with rheumatoid arthritis. Southworth compared arthritic with ulcer patients: the former showed more tension in the trapezius muscle but no difference in the frontalis, and in a sentence completion test there were no differences between the groups as regards dependent or aggressive needs. These results support neither a specificity theory nor a constitutional one, but the constitutional position continues to have an active champion in Mirsky (53), who again reminds us that ulcer patients have significantly high levels of pepsinogen in the blood, a sign of the capacity of the gastric mucosa to secrete it, and that "high pepsinogen secretors represent that segment of the population with a maximal secretory capacity which is most likely to develop duodenal ulcer when exposed to precipitating circumstances." Similarly, pernicious anemia occurs in people with naturally low pepsinogen and poor absorption of vitamin B₁₂. Mirsky proposes testing large numbers of people and then following them to see whether or not the incidence of later disorders follows predictions made from constitution.

In line with Grinker's idea of the early conditioning of autonomic patterns of response to stress, Grossman & Greenberg (30) report a fresh analysis of autonomic responses in newborn infants. Their results are in general agreement with those reported earlier by Richmond & Lustman (62),

and thus strengthen the evidence for great individual differences in the first few days of life. "It appears possible," they say, "that every newborn infant may have at least one 'vulnerable' component of the autonomic nervous system whose response to environmental stimulation shows a capacity for more lability." We can see in these studies the precursors of the individual "autonomic constitution" studied by Lacey and others in the Fels Foundation children, and it is not hard to imagine that such patterns, evoked and organized by the major stresses that happened to occur in infancy, would be activated again, complete with their vulnerably labile elements, during long-continued psychological stresses in adult life.

DELINQUENCY AND PSYCHOPATHIC PERSONALITY

The concept of anxiety has proved so central in understanding abnormalities of behavior that clinicians are apt to be bothered by disorders from which anxiety seems to be absent. When mental health has been so widely identified with freedom from crippling anxieties it is paradoxical, not to say annoying, to find delinquents and psychopaths enjoying this freedom while constituting a menace to society. Yet there seems to be little doubt about that freedom, which was reaffirmed last year by three investigations. Sherman (71) reports that psychopaths show significantly better retention of meaningful and of nonsense material than do neurotic or normal subjects, a finding which may well signify their greater freedom from anxiety. Lykken (49), approaching the matter more directly, predicted that sociopathic personalities (as he calls them), when compared with neurotics and normal controls, would score lower in manifest anxiety, in rate of anxiety conditioning, and in rate of avoidance learning; all three predictions were confirmed. Shapiro (70), who compared aggressive boys and withdrawn boys 6 to 10 years old on projective material involving family situations, found the aggressive boys more optimistic, more confident of favorable outcomes, more trustful of their own resources—in short, quite the picture of ego strength and rugged individualism, yet problematically aggressive.

These results, of course, are not incongruent with the findings of Redl & Wineman in their books, *Children Who Hate* (1951) and *Controls from Within* (1952), reissued last year in a single volume entitled *The Aggressive Child* (60). And it is interesting that Kaufman & Heims (37), in a clinical study of juvenile delinquents, pursue one of the easily overlooked themes in those books, that of "islands of identification" and brief periods of security in a generally chaotic family history. Delinquents have often suffered immense rejection, but not so uniformly and so early that they failed to survive. Kaufman & Heims advance the idea that at the root of the matter lies a depressive nucleus based upon early separation. Delinquent behavior thus has, in their view, a very deep root in anxiety, though on the surface it is a bold way of combatting depression and restoring an injured body image.

AFFECTIVE DISORDERS AND THEIR TREATMENT

In striking contrast to the enormous current interest in schizophrenia is the virtual silence now prevailing in the literature with respect to manic and depressive disorders. Quite possibly this has happened because the affective disorders are such heavy beneficiaries of the new somatherapies, electroconvulsive therapy (ECT) being much recommended for depressive states and tranquilizing drugs for manic and agitated conditions. Reports on ECT are now showing clear signs that the stage of maturity has been reached in the cycle of discovery. Carpenter (7), for example, undertakes to specify more precisely the indications for electroshock by considering in his patients the direction of aggression and its acceptability to the ego. In his report he shows that results are good when there is ego-alien extrapunitive-ness, poor when there is ego-syntonic intrapunitive-ness.

In a similar vein Regan (61) examines the effective utilization of ECT in conjunction with psychotherapy and specifies a complex set of relationships. Effectiveness is greater when depression is involved but it is also related to psychological constellation.

Thus, affective disorders responded best to ECT; paranoid reactions and psychoneurotic reactions responded well; and schizophrenic reactions poorly. Influencing these results, however, was the fact that depression associated with agitation, anxiety, fear, or sexual content yielded better results, and depression associated with hostility, guilt, or body concern yielded poor results, in each diagnostic category. . . . Optimal efficiency was obtained only when ECT was indicated by both diagnosis and psychopathological state (61, p. 356).

There is more to be worked out, but these reports are encouraging steps toward a precise and discriminating use of a technique which at first was treated as a general panacea. As a further example of maturity we might mention the excellent statistical survey by Staudt & Zubin (74) of reported results of the shock treatments and lobotomy, though this particular paper is limited to results with schizophrenia.

Naturally the marked success of somatherapies has given confidence to those who believe that manic-depressive illness is fundamentally a somatic disorder. This is the burden of a new book by Kraines (42) and of a paper (43) distilled from it. Kraines marshalls evidence to show that the diencephalon-rhinencephalon-reticular system is the only portion of the brain we need assume to be primarily involved or to be pathologically influenced in manic-depressive disorders. Involvement of the cerebral cortex is a purely secondary phenomenon; the psychodynamic mechanisms and reaction patterns that appear in the patient's behavior are only symptoms which disappear when the pathological physical condition improves. This is putting it strongly, but if we look for studies which investigate the life history and family constellation of manic-depressive patients there is only the paper by Gibson (26), which has a relatively small yield. Contrasted

only with schizophrenics, the manic-depressives come from a background marked by concern for social approval, striving for social prestige, an atmosphere of envy and competitiveness wherein the patient, as the object of envy, has counteracted with a pattern of under-selling himself. No psychogenic theory seems likely to emerge from these mainly sociological findings.

The ataractic or tranquilizing drugs have been valuable in dealing with the excited and agitated aspects of manic-depressive disorder, but they are not indicated for straight depressions. Many workers have been hoping that ECT could be put away on the shelf along with lobotomy by the discovery of a drug which would specifically elevate the spirits of depressed patients. Such a drug has lately been put on the market in the form of marsilid (iproniazid), but the reports on its use scarcely entitle it yet to be considered a wonder drug. Ayd (1), for example, finds the effects irregular and unpredictable, with benefit showing in less than half the patients with whom it was tried. Schopbach (69) cites good results with mild depression alone but not with depressions involving schizophrenic or other complications. Ferreira & Freeman (17) likewise claim rather limited good results with psychotic depressions. A much more hopeful note, however, is sounded by Robie (64), who reports success with 46 out of 65 patients, and who believes it practicable to control most of the undesirable side-effects reported by others. Marsilid must be given for three or four weeks before showing its full effects, which means that it cannot altogether replace ECT, but it is certainly much more satisfactory for use on a maintenance basis. Its value with depressions cannot yet be fully estimated, but already it has been applied to another problem, the vexing one of mute catatonics who refuse to eat. Breitner (5) reports that no tube feeding has been required at his hospital since marsilid treatment was begun with these patients.

TRANQUILIZING DRUGS

Turning now to the ataractic drugs, the outstanding scientific breakthrough of the decade in psychiatry, we find that there is still a deluge of papers which belong in the buoyant and expansive stage of the cycle of discovery. One might say facetiously that the drugs have had a curious double action, that of calming the patient while elating the physician. But facetiousness dies when we contemplate the benefits obtained through the use of ataractic drugs. Brill & Patton (6) point out that the patient population of the mental hospitals of New York State, which for some time had been increasing by 2000 or more every year, dropped by 500 because of the increased discharge rate for the first year in which chlorpromazine and reserpine were in large-scale use. Within the hospitals still further consequences could be observed.

One of the most outstanding values of the use of tranquilizing drugs in mental

institutions is reflected in the data on restraint and seclusion. In 2 years' time these figures were reduced by 75 per cent. This is only a mathematical expression of what has been a revolution in the care and treatment of mental patients (6, p. 516).

It is interesting to reflect that Pinel's mission in striking the chains from the insane has only now come close to fulfillment.

The subject of tranquilizing drugs is well-summarized by Wortis (80) in a review from which we learn that third-stage doubts and criticisms have begun to be heard and that a fourth-stage appraisal may not be far away. It is already clear that chlorpromazine is the most effective of the drugs, even though its considerable toxicity leads in some cases to undesirable side effects. The derivatives of rauwolfia, on the other hand, of which the best known is reserpine, have not stood up so well, a point which has been established in a well-designed study by Malamud *et al.* (50). These derivatives have a rather small initial therapeutic effect, and they are not suitable for long term use with patients whose blood pressure is on the low side. As experience lengthens, new problems inevitably arise. Wortis notes that the World Health Organization has just added the tranquilizers to its list of habit-forming drugs, and he cites a number of reports which tell of unfortunate consequences, particularly in the functioning of the liver, when patients are kept indefinitely on maintenance doses. With chronic patients the ataractic drugs do not usually produce an effect so lasting that they can be discontinued without relapse. It is plain from Wortis's article that perfect drugs for long-term treatment have not yet been found, and that even for initial treatment the matching of drug, dosage, and patient is still very much a matter of trial and error.

One of the disheartening features of lobotomy and the shock therapies was their failure to make their expected contribution to scientific knowledge. Some valuable information was obtained, of course, but on the whole we still do not have good explanations of the effects produced by these techniques. Perhaps the ataractic drugs will fare better, but at present we are certainly still in the same position of ignorance. An interesting suggestion comes from Porteus & Barclay (58) to the effect that continuous treatment with chlorpromazine produces effects comparable to those of lobotomy. Patients treated for two years with 300 mg. of chlorpromazine each day were less self-concerned, less anxious, less aggressive than they had been before, and they showed significant deficits in the Porteus Maze experiments. However, a speculative paper by Gordon, Zeller & Donnelly (28) puts emphasis rather upon a similarity between chlorpromazine and insulin shock, suggesting that both have a significant effect on adrenal functioning. It is a hope, but still only a hope, that the new drugs will help in the campaign to surprise the living organism's well-guarded biochemical secrets.

SCHIZOPHRENIA

We come now to the disorder which bulks largest in the year's reports on abnormalities of behavior. Research has been highly active on both the bio-

chemical and psychodynamic fronts, and the whole theory of the disorder has come up for extensive reconsideration.

General nature.—The initial break-through in the modern understanding of schizophrenia may be attributed to Kraepelin on the biological side, Jung on the psychological, and especially Bleuler, who combined the best features of each. Respect for Bleuler's tremendous accomplishment is still on the increase. Kinross-Wright & Kahn (39) remind us of his conception of schizophrenia, which they believe to be still wholly valid. The fundamental disorder is seen most clearly in schizophrenia simplex and consists of a particular type of alteration in thinking, feeling, and relation to the external world. The more dramatic symptoms and the schizophrenic personality patterns are secondary developments which should not be confused with the basic disorder.

Bleuler is the hero also of a paper by Freyhan (24), who uses statistics from the Delaware State Hospital, the only psychiatric hospital in the state, to show that in the course of 51 yr., despite improvements in methods of treatment, despite even the tranquilizing drugs, Bleuler's expectations concerning the proportion of recoveries and of deteriorations have been regularly realized. We cannot entertain the illusion, Freyhan insists, that treatments have increased the number of good recoveries or halted the inexorable 22 per cent destined for severe deterioration. In his opinion we have not advanced beyond Bleuler and may even have lost ground by regarding schizophrenia "as a uniform object of research and treatment" instead of as a group of disorders having very different severities and prospects.

Perhaps Bleuler is not to be surpassed, but other workers continue to search for general formulations which will cast new light on schizophrenia. Kline (40), who reminds us that the issue of one vs. several disorders is still unsettled, proposes a general hypothesis centered around the disorganization of systems which normally work in an appropriate balance. Pollack & Goldfarb (56), in a clinical study of severely disturbed children, put the emphasis on subjective disorientation in an emotionally overwhelming environment, noting that some of the peculiar behavior represents an attempt to maintain orientation. Goldberger (27) advances an interesting hypothesis in which the blocking of kinaesthetic sensation plays a prominent part. He believes that kinaesthetic awareness of one's own body and of muscular movements which effect contact with the environment are central ingredients of the sense of reality, and he notes that schizophrenic patients have poor awareness of their own bodies as well as of the outside world.

Follow-up studies of childhood schizophrenias may be expected to throw some light on the general nature and stability of the disorder. Freedman & Bender (21) report on six cases who are now fully adult. Chosen because of availability, these cases may overrepresent the less recovered who have not slipped out of medical circles; at all events it seems clear that they are still schizophrenic, even though they manage somehow to get along in the community.

Eisenberg (12) has surveyed the literature on follow-ups. The average of 33 per cent who deteriorate compares unfavorably with Bleuler's expectation for adult patients, and the 25 per cent who attain a moderately good social adjustment during adolescence is less than Freyhan's Delaware figure, which showed that 35 per cent of adult patients leave the hospital after one attack and do not return. The figures are not precisely comparable, but we might expect poorer averages among patients who have been afflicted so early in life. Eisenberg's figure for recovery is strikingly confirmed by Errera (15) on the basis of a 16-year follow-up of young patients seen in an outpatient clinic. His statement that the other 75 per cent remain severely handicapped is a grim reminder of the severity of these disorders.

Two papers, however, take issue with current concepts of childhood schizophrenia and suggest caution in interpreting these figures. Despert & Sherwin (9) reject two of the criteria often used in diagnosis—atypical development and symbiotic dependence—on the ground that they are much too common to be specific for one disorder. They accept as diagnostic categories "autistic," for children who show Kanner's syndrome from the start; "schizophrenic," for those who develop the syndrome later; and "psychosis with mental defect," which of course involves another element besides autism. This seems to be an attempt, in line with Bleuler, to stick to primary symptoms. In a similar vein, Mosse (54) is sharply critical of the free use of schizophrenia as a diagnostic label in childhood and of the therapeutic proposals that go with it, sometimes including shock treatments. She questions the identity of childhood and adult conditions, pointing out that adult schizophrenic patients are almost always reported to have had quiet and unobtrusive rather than worrisome autistic childhoods.

The problems surrounding schizophrenia are complicated by the possibility that a fundamentally schizoid condition may be compatible with a life pattern which easily passes for normal. This idea was made familiar years ago by Kretschmer and was carefully worked out by Zilboorg in his concept of ambulatory schizophrenias, a concept so widely misused that Zilboorg (81) was obliged to clarify it again last year. Ambulatory schizophrenics, he points out, are people who live an externally normal life but with a fundamental autism which prevents a true affective contact with the environment. They thus tend to appear detached and a little preoccupied; they have trouble deciding what interests them; they marry to prove that they are capable of being married; they have children to demonstrate their parental maturity; and they love their children narcissistically. Are people of this sort "sick" in some legitimate sense of the word? Are they deficient in reality testing? Are they more than slight exaggerations of normal human narcissism?

Proper conceptualization is not easy, nor is it made easier by a shrewd and wise book entitled *The Introvert* by Meares (51). This book describes

the life problems of the "individual of introverted personality," the "stranger whom we know, but whom we do not understand," the child who seems always "vaguely apprehensive" and who "feels that he is different from the others." Meares tells us that introversion shades directly into schizophrenia but is not itself schizophrenia; indeed, he sees the introvert as a potentially superior type, capable of a much greater development and contribution than the average person. Many readers will probably accept the idea that a sensitive soul who has a hard childhood may develop unusual depths of understanding, but they may have difficulty with the notion that such a person has a touch of the disease called schizophrenia. The conceptual difficulties embedded in the significant contributions by Zilboorg and Meares do not permit us to suppose that our grasp of the general nature of schizophrenia is yet firm.

Biochemical aspects—The work of Heath and his associates at Tulane University has been widely publicized in newspapers and news magazines. It confronts us here in the more sober form of scientific reports, of which the two most interesting (Heath *et al.*, 31, 32) deal with the effects of taraxein. Taraxein is protein substance that can be extracted from the serum of schizophrenic patients but not from normal people. The present reports describe what happens when this substance is injected into non-psychotic subjects, prison volunteers being used for the purpose with artifacts prevented by a double-blind design. All 20 subjects who received taraxein developed primary schizophrenic symptoms consisting of thought blocking, autism, depersonalization, a blank look, and dull facial expression. Secondary symptoms which appeared in one or more of the subjects included catatonic stupor and excitement, hebephrenia, delusions of persecution, grandiosity, and auditory hallucinations. The visual symptoms characteristic of mescaline intoxication and the autonomic reactions produced by lysergic acid diethylamide did not appear. Heath and his associates believe that the latter drugs have an effect resembling toxic psychosis rather than schizophrenia. In a small additional experiment (32) rapid blood transfusions were made from schizophrenics to normal subjects, who developed distinct but mild symptoms which passed within an hour.

These studies suggest that the specific biochemical cause has been located, but unfortunately they are not confirmed by other workers. In the discussion following the public reading of the second paper by Heath *et al.* (32), Kathleen Smith reports injecting five volunteer prisoners with taraxein; "none exhibited either the primary or secondary symptoms described by Bleuler." Hoagland (33) writes that an attempt to duplicate the Tulane experiments in his Worcester laboratory brought wholly negative results. Freedman & Ginsberg (22) reversed the transfusion procedure by removing about 60 per cent of the blood from patients and substituting blood from normal donors. This had no effect on the three adult schizo-

phrenics with whom it was tried, though possible slight improvement for a few days was noticed in a child patient. Hoagland's report (33), which summarizes a great deal of current work, expresses grave doubt that the biochemical secrets of schizophrenia lie in taraxein, adrenal peculiarities, pineal extracts, mescaline, lysergic acid, or serotonin in the brain. Experiments to date convince him that "we are still groping pretty much in the dark." Rubin (65), reviewing the literature on lysergic acid, discusses several hypotheses that have been offered to explain its effects on behavior, but he ends with a similar statement of uncertainty.

The theme of cerebral metabolic deficiency continues to attract investigators. Wertheimer & Jackson (78) report that schizophrenics show significantly less visual and kinaesthetic figural after-effects than do normal subjects, which suggests a lower metabolic efficiency of the brain in the patients. A study by Finkle & Reyna (18) seems to be in harmony with this idea. They gave maintenance doses of L-glutavite to long hospitalized, elderly schizophrenic patients, masking it in routine glasses of tomato juice. L-glutavite is a variant of the glutamic acid which has been found beneficial with mentally retarded children, though not to an extent to overcome their retardation. The majority of the patients responded by becoming more active and interested; some attained a satisfactory level of social behavior, and nearly all reached at least a point where they could be handled by aides and volunteers—a matter of great practical importance when hospitals are short of more highly trained personnel. These are samples of encouraging reports, but on the whole it must be admitted that the study of the biochemical aspects of schizophrenia is in bad need of a break-through.

Family environment.—Stalking the "schizophrenogenic" mother continues to be a favorite pastime with clinicians. Past research has given her numerous and conflicting outlines, but it is apparently still easy to suppose that she is the cause of schizophrenia in her offspring. Two papers within the year cast her in somewhat different lights. Dworin & Wyant (11), who observed mothers in group psychotherapy with their adult schizophrenic sons, paint a familiar picture of dominance, smothering, taking the initiative, and keeping the child dependent. This finding, of course, can be interpreted in exactly the opposite way: the mothers' behavior is the consequence of living for many years with a passive, unresponsive schizophrenic child. Whatever the interpretation, the paper is valuable for its report of direct observations, but the authors founder on a methodological reef in that part of their research in which the F (California Authoritarian) scale was administered to mothers of schizophrenic patients. As a control group they chose hospital volunteers, surely an unwise choice if the group is intended to represent the general population in qualities measured by the F scale. This suggests the general reflection about clinical research that perhaps the control group, when needed, should be regarded as the major technical

obstacle, the problem to be solved first before one decides to go ahead. Certainly it is not satisfactory to use people who just happen to be around the hospital.

Be that as it may, the picture of "authoritarian" mothers presented in this report is somewhat different from the portrait drawn by Kaufman and associates (36) in a study based on therapeutic work with schizophrenic children and their parents. Here the parents are "rigidly controlled people who maintain a façade of independence and demonstrate a stereotypy of life patterns" (36, p. 684). This pathological adjustment is threatened by the child's presence, and in the resulting interaction the child learns that destructiveness is inherent in human relationship; he fears annihilation and dares not establish a sense of identity different from the concepts the parents have of him. Therapists are warned in the article that they must not be frustrated by the limited capacity of these children to take what is offered, be it gift or affect. Again one is entitled to wonder whether the parents may not have experienced persistent frustration over this quality in their child. The problem of interaction needs more exploring than it is given in this article.

That elusive creature, the father of the patient, is increasingly being lured into clinical studies to share the pitiless spotlight with his hard-pressed wife. Eisenberg (13), reviewing the records of 100 autistic children, deduces from the rather full notes that at least 85 of the fathers were "obsessive, detached and humorless individuals" who sound autistic in their own right and who fit rather nicely into Zilboorg's concept of ambulatory schizophrenias. Eisenberg draws his conclusions with due caution, reminding us of the fact that practically all parents of autistic children have other children who are not autistic, and it is perhaps on this account that he does not mention the inheritance of constitutional peculiarities, which would otherwise seem an appropriate hypothesis to explain the results.

A more intensive study is reported by Lidz *et al.* (46), who detect five types of fathers in a series of 14. These types are not peculiar to schizophrenic patients, but in the family environments here studied they are unhelpful in various ways. They contribute to an atmosphere of irrationality and distortion of reality which inclines the child toward what he may later use as a method of extreme withdrawal. Included in this paper is a challenge to the view that schizophrenia involves regression to the early oral level. It is held that the critical symptoms reflect later stages and that one should turn to the psychosomatic patient for true early oral regression.

It is reassuring to find that several workers are using the concept of interaction patterns in families rather than the questionable cause-effect model of parent influencing child. Behrens & Goldfarb (3) report a study in which interaction patterns were rated by observers; differences were found between the families of schizophrenic children and "contrast families"

with no sick child. Lichtenberg (45) describes two cases in which exalted family expectations were part of the pressure that produced schizophrenic breakdown. Jackson (34) characterizes schizophrenia as a "family-borne" disorder, a not inappropriate reaction to a peculiar pattern of relationship and communication. Lidz *et al.* (47) pursue a similar theme in their study of the transmission of irrationality. The idea that the disorder of thought, as well as the affective aspects of the illness, can be seen as a derivative of the family situation is the subject of a short but searching paper by Flavell (19). Maintaining that "etiological accounts of the formation of schizophrenic thinking should show specifically how cognitive development itself becomes affected by pathogenic early interpersonal relations," he discusses several possible patterns and relates them to the thought changes experienced at the onset of schizophrenic episodes.

Although the evidence for a psychogenic account is constantly growing more bulky and impressive, it is still far from meeting the highest standards of scientific method. Those who enjoyed Orlansky's (55) article nine years ago will take similar pleasure in a paper by Stevenson (75) which expresses doubt that any of the assumed relationships between early events and later personality have ever been proved. Even more impressive than this direct attack is the implication of a paper by King (38), who examines the problem of proof in experiments with animals, where proof should be easiest. Few studies satisfy King's high standards by dealing adequately with all the relevant parameters, such as age at time of experience, age at time of test, duration or quantity of early experience, type or quality of experience, and persistence of effects. The clinical investigator may well be discouraged to realize how little he knows about relationships he would like to take for granted, but the year's literature, with its conspicuous lack of agreement and conflicting suggestions, cannot fail to leave him dissatisfied with current progress in understanding the psychodynamics of development.

EXPERIMENTS WITH HOSPITAL CARE

The rapid rise of interest in the mental hospital as a social system and as a therapeutic milieu is reflected in several books and a large number of papers. Greenblatt, Levinson & Williams (29) present a series of papers prepared by themselves and other distinguished authorities for a symposium on the subject, and another very substantial contribution comes from Leighton, Clausen & Wilson (44). Here we shall consider certain studies which deal with the care of chronic hospitalized patients and which have a significance beyond their obvious practical value. They bear upon problems of chronicity and deterioration, and they raise once more, as we shall see, the eternal problem of biochemistry vs. psychodynamics.

First there is the book by von Mering & King (76), based upon extensive observation of experimental wards all over the United States where

something beyond routine custodial treatment was being undertaken. If one is forewarned not to expect miracles, this is a most encouraging document. It shows that with adequate staff and hopeful leadership the condition of the majority of chronic patients can be significantly improved. This improvement may consist of little more, in the most severe cases, than becoming able to feed themselves, manage excretory needs, and keep dressed; but it always betokens an increase of awareness, interest, and interaction, and in more favorable cases it may lead over into gradual return to the community. The patients do more about the ward, decorating it, preparing food, giving little parties, taking some pride and satisfaction in their quarters and their membership. A similar experiment is reported from Glasgow by Freeman, Cameron & McGhie (23). It is preceded by a long and faithful account of psychoanalytic theory, which in this case, however, seems to be superfluous, for the methods used and the results obtained are scarcely to be distinguished from those reported without any particular theory by von Mering & King.

To be placed with these books are two papers by Scher (67, 68), who introduced into a deteriorated ward a very definite atmosphere of required tasks and staff expectations. The insistence upon orderly and appropriate tasks having to do with self-care and operation of the ward had a good effect upon the patients. "An awareness of the self in relation to task and both of these in relation to other persons seemed to develop." All of these contributions show that something can be done for chronic patients by paying attention to them, taking them seriously, and rewarding them with respect for whatever they can accomplish.

Shifting the scene quickly to drug therapies, we make a disconcerting discovery. Porteus (57) reports that, when chlorpromazine was introduced as regular treatment in a general ward, the patients improved in very much the same way. Ratings of ward behavior after 18 weeks showed improved alertness, interest, and social responsiveness in 60 per cent of the patients; of these, 11 per cent improved with a placebo, leaving a 49 per cent victory for chlorpromazine. We have already mentioned the work of Finkle & Reyna (18) with L-glutavite, where the description of improvement contained practically the same words.

Can it be supposed that drug therapy is actually milieu therapy in disguise? This question is raised by Rashkis & Smarr (59), who first used milieu therapy with a group of 48 female catatonic patients, obtaining improvement in 39, then added a variety of drugs and found no significant further change. The authors mention that they were not enthusiastic about the drugs and suggest that this prejudice might have influenced the results. As their principal tranquilizer they used reserpine, which has not been doing particularly well in recent trials. Thus we are left with a curious uncertainty about what it is that improves chronic hospital patients. One is

almost tempted to think of a single scale of improvement on which a patient's position can be bettered by a given unit of chlorpromazine, or L-glutavite, or respect, or structured tasks. This way, no doubt, lies psychosomatic madness, but the lesson of the year's reports on experiments with hospital care must be that we still stand rather far from understanding what we are doing in problems of this kind. At least there is cause for rejoicing that the lives of chronic patients can be made a little more meaningful, even if the theory of that improvement still escapes us.

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THEORY AND TECHNIQUES OF ASSESSMENT^{1, 2}

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The indigenous actuarial-empirical approach to assessment is opposed, as regards global assessment, by the clinician who lets his unconscious mind resonate to that of his client, and as regards test theory, by advocates of construct validation. If there is a trend for this year it is toward sophisticated integration of theory and data. The level of discussion is higher than that of a few years ago, thanks, among others, to Meehl, who on several occasions in recent years has clarified the issues in addition to embracing two extreme positions, to wit, a "cookbook" or actuarial approach to assessment and a construct validation approach to test theory.

GLOBAL ASSESSMENT

Actuarial assessment is invariably based on prior correlation of assessable characteristics with predicted performance, as Holt (80) pointed out, while many of the studies quoted in Meehl's *Clinical vs. Statistical Prediction* have pitted sheer guesswork on the part of the clinician against actuarial prediction. The clinician, too, must have data about requirements for effective performance and their manifestations in prior assessments, not only to be judged fairly, but even to be sensible. "No matter how remarkable clinical judgment may sometimes be, it can never create information where there is none" (80, p. 2). The Menninger project (80) for assessment of psychiatric residents made use at various stages of 3 types of prediction which Holt called (a) pure actuarial, (b) naive clinical (without prior study of prediction to a criterion), and (c) sophisticated clinical (with prior study of prediction but with the final assessment instrument being the clinician who does not necessarily apply the same rule to every case). In the naive clinical design, psychiatrists on the basis of an interview and psychologists on the basis of projective and other tests recommended whether to accept the applicant and judged how good a psychiatrist he would become. Both judgments had some validity: validity of predicted efficiency of accepted applicants, hence a restricted range, represented by *rs* of about .25. In the actuarial and semi-actuarial design, scoring manuals were devised for converting the projective tests to instruments for pre-

¹Survey of literature pertaining to this review was completed in April, 1958.

²The following abbreviations are used in this chapter: MMPI (Minnesota Multiphasic Personality Inventory); MAS (Manifest Anxiety Scale); TAT (Thematic Apperception Test).

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dicting efficiency as psychiatrists; in some cases purely actuarial prediction based on Rorschach and other signs was attempted. Purely actuarial formulas had no validity on cross-validation. The scoring manuals yielded predictions of psychiatric competence with validity coefficients in the neighborhood of .2 or lower. In the sophisticated clinical design, psychologists, after preliminary studies of the qualities which differentiated most from least successful psychiatric residents, made their predictions using all available data. Validities of about .5 were achieved in this phase of the study.

Interestingly, rating of "likability" by psychologists, intended as a suppressor variable, was the best single predictor of success. This finding is dangerous because everyone wants to believe that his personal likes are not arbitrary but represent the order of nature. What if the selection of psychiatric candidates falls to someone who just doesn't like Negroes, or women, or Hindus (all of whom, incidentally, were excluded from the study)? Psychiatry already has seen the stone that was rejected become the cornerstone. The most plausible explanation of the goodness of likability as a predictor is that it contaminates the criteria (ratings by supervisors and peers), a possibility mentioned but not stressed by Luborsky & Holt (116). Prediction of combat performance and adjustment of Air Force pilots, only modestly successful, turned up likability as a particularly stable trait, but it had no merit for prediction [Trites & Sells (193)].

Likability typifies the moral hazard of clinical assessment; there is an equal hazard in actuarial assessment. Seagoe (157) followed up students whose fitness for teaching had been questioned. As a group they had succeeded in becoming teachers, perhaps mediocre ones. Oddly, those the judging committee rejected did better than those not rejected. A higher proportion of those with visual and neuropsychiatric referrals got low grades in practice teaching than those referred for obesity (!) and other causes. Therefore, Seagoe seems to imply, the former should have been rejected and possibly kept out of the profession. Should assessment close the doors which rehabilitation efforts are trying to open?

Views similar to Holt's were stated by Super (181) and Holzberg (82). Both stressed the importance of accumulating knowledge before attempting prediction and of expressing every prediction as a probability. Meehl (126) and his critics appear to be essentially in agreement on these issues: when a formula is available, which will be seldom, the clinician should use it instead of intuition (126, 181). What the clinician does best is to detect configurations in multiple variables; he is out of his element when required to express himself in terms of rating scales or to predict criteria such as grade-point averages (80, 82).

Supporting the importance of feedback, Thomas & Mayo (188) demonstrated that knowledge of success of predictions led to significant improvement in vocational counseling. Studies of prediction of Army leadership resulted in coefficients similar in magnitude to those of the Menninger

study, with .5 representing optimal prediction, even where predictor and criterion were both ratings and hence may have shared error variance [Drucker (53)].

CONSTRUCT VALIDITY

Emphasis on the clinician's accumulation of knowledge of what in his instruments correlates with what traits and destinies helps to explain the resistance to change of, e.g., the Rorschach and the original nine scales of the Minnesota Multiphasic Personality Inventory (MMPI) despite good arguments for revision of these tests. New projective instruments have poor chance of adoption. In contrast, new tests whose entire meaning is carried by a single numerical score, such as the Taylor Manifest Anxiety Scale (MAS), come into wide usage without adequate validation. Jessor & Hammond (85), reviewing the literature, seriously questioned the construct validity of the MAS, as did Jenkins & Lykken in the 1957 *Annual Review*.

Construct validity, proposed in the *Technical Recommendations for Psychological Tests and Diagnostic Techniques* and elaborated by Cronbach and Meehl as a method for test evaluation, was developed by Jessor & Hammond (85) as a program for test construction. They stated that the form of the scale, item selection, item content, and nature of the responses should be coordinated to the theory giving rise to the test. Similarly, Loevinger (114) analyzed construct validity into a substantive, a structural, and an external component. Evidence under all three headings converging on a single theory or hypothesis is minimal evidence for construct validity. There is a common theme in the above expositions: to establish construct validity one must account for all of the evidence in relation to a test, and that evidence must come from distinct and disparate sources.

Construct validity has been used as an honorific label for evidence (confirmation of a single hypothesis or of a single connected set of predictions) falling far short of these standards or even for failure to establish external prediction. Predictive validation construed as a program of test construction invariably means maximizing prediction of a single criterion. Because of the amenability of this program to algebraic development, it will probably remain the psychometrician's delight. Construct validation implies a program for simultaneous maximization of as many predictions as possible, thereby justifying the inference of an underlying trait. It cannot be reduced to an algebraic or mechanical program. The term has probably not appeared in *Psychometrika*. But it is travesty to construe advocacy of construct validation as opposition to external validation.

Cattell (26) used the term construct validity in connection with a project in which he was already engaged: construction of a Universal Index of personality factors established by rigorously defined factor analytic procedures. His questionnaire, the 16 Personality Factors Test, is intended to tap factors thus established. This year he presented a discussion of the factors and of the studies used to establish them. The work summarized represents arduous fulfillment of high standards; nonetheless, the standards

are different from those proposed by other protagonists of construct validation. For Cattell the construct validity of a test is a number, its correlation with a pure factor (25, p. 232). Dahlstrom (41) criticized Cattell for claiming construct validity for his questionnaire on no more evidence than reproduction of factor loadings of items in a new sample with no nontest correlations to verify claimed interpretations. The issue between Cattell and proponents of construct validity is more than a contest to determine just who gets the name; their programs are at variance. Cattell calls for factor analyzing separately data from ratings, questionnaires, and objective tests (mostly physiological) of personality. Factors determined in one medium are matched with those in another by a complex process of ratiocination. He anticipates that this process will continue for some years. By contrast, Campbell & Tyler stated: "*Construct validity* is epitomized by the correlation between two or more specifications of a construct maximally *different* in apparatus or method" (23, p. 91). The hazard in Cattell's program is that one may spend years factoring artifacts of the several methods before one learns how little variance is shared with measures in other media. None of the factors in Cattell's book was considered to be an artifact. Although recognizing in principle that questionnaire responses may signify something other than what they appear to say, Cattell invariably interpreted the responses of his own subjects literally.

Response biases in inventories will be discussed later. A study in which college students were asked to rate their parents and themselves on 40 traits provides unwitting illustration of artifact in that medium (89). The ratings were combined, subtracted, and correlated in a variety of ways. Only those correlations were reported which were predicted to be high, and usually they were. Almost the only nonsignificant correlations were those between variables supposed to measure the same trait! What the results demonstrate is not that "identification with parents, feelings and attitudes to parents, and self-esteem all cohere as a syndrome" (89, p. 380), however true that may be, but that when using a rating scale, college students tend to describe themselves, their parents, and, as it happens, almost everyone else, more or less similarly. Campbell & Chapman (22) showed that ratings by Air Force men of father, boss, younger brother, and work subordinate tended to correlate around .5. They found only a slight (but statistically significant) tendency for attitudes toward father and boss to be more similar than attitudes toward father and subordinate and toward boss and younger brother. This study, along with others carefully designed by Campbell and co-workers, showed only very slight generality of attitudes toward authority as revealed in ratings and other easily obtained measures, compared with unexpectedly large generality in attitudes toward everybody. Two consequences should be wariness of the artifacts in ratings and wariness of the banalization of psychoanalytic concepts, the representation of the deepest experiences of psychoanalysis by easily obtained ratings or by the difference or correlation between ratings. Crow & Hammond (38) showed

that in use of ratings to measure interpersonal perception, response sets were considerably more consistent over time than differential accuracy. Self-ratings appear to be especially poor criteria of adjustment [Beilin (9)] and of improvement in therapy [Kelman & Parloff (99)]. Ratings and interpersonal perception will not be further considered in this chapter.

PREDICTION TO A CRITERION

Construct validity has by no means carried the day; prediction to a single criterion remains a major line of research and the chief method of test construction. The criterion problem continues to be discussed [Ryans (147); Wherry (202); Thorne (189)]. Dailey (42) suggested that life-history data should be the best source of predictors because they are part of the criterion. His point is illustrated by Sopchak's (173) finding that college grades are predicted better by high school average than by any test. Until tests can do better than easily obtained life-history data, assessment as an industry would appear to be uneconomic.

A study of American prisoners of war returning from Korea concerns the kind of criterion a future Office of Strategic Services might be called on to predict. These prisoners were given a large variety of tests shortly after their repatriation. They were divided into three groups: collaborators, active resisters, and neutrals. Collaborators and resisters were not significantly different on any variable; on the few items where they differed from the neutral group, the difference was in the same direction [Schein *et al.* (154)]. If we cannot distinguish such differently behaving people after the fact, how can we predict before the fact? The intransigent unpredictability of people remains the universal attenuation factor.

For years psychological literature has propounded the merits of pattern analysis and configural scoring. The valuable patterns were almost invariably to be chosen according to predictive validity. Large scale projects have been started to improve well-known tests by such methods, using large numbers of cases and in some cases electronic computing machines for pattern search. Where are the results? Neither McQuitty's (125) article nor bulletins describing the studies of Lubin (115) and Saunders (150) reveal any consistent superiority of configural over linear scoring models. The fact that the other studies remain unreported presumably means that the actuarially chosen patterns failed to hold up on cross-validation or predicted no better than linear keys.⁴ Several studies reported that scoring keys, not configural, constructed to have predictive validity for one or another purpose, failed to hold up on cross-validation (30, 39, 80, 175, 186, 199). Similarly, Gage (63) showed the superiority of an *a priori* to an empirical key.

A criterion-oriented problem of continuing interest is detection of brain damage by psychological test (properly, discrimination rather than prediction). Tests based on the Archimedes spiral aftereffect apparently dis-

⁴ I am indebted to Dr. Ardie Lubin for calling these facts to my attention.

criminate well between organics and normals as groups but have less diagnostic utility than was once hoped (44, 176). Conflicting reports on whether reporting the spiral aftereffect is correlated with age or IQ are probably explained by different selection of Ss (1, 66, 74); in any case, the suspicion is growing that score differences are accounted for by differences in verbal report rather than differences in perception [Aaronson (1), Goldberg & Smith (66)]. Stilson, Gynther & Gertz (178), reviewing several previous studies, stated that, despite its sensitivity to group differences, whether the test increased the number of correct discriminations depended on the base rate of organics in the population studied. They quoted Meehl & Rosen (126a) on the problem of how additional information can worsen discrimination. Cureton, however, (40) called Meehl & Rosen on the point: if the added data are properly used, discrimination cannot be poorer. Cureton's note, nevertheless, underscores the main argument of Meehl & Rosen: unvarying cutting points should not be set for any psychometric device. The optimal cutting score must be set anew in each clinical situation, depending on the proportion of patients in the two classes. While different cutting scores were used with the spiral aftereffect test, none of the studies showed a grasp of this point. To accept the point would, indeed, make tests almost useless for clinicians operating in settings lacking statistical feedback. Is some compromise possible between practical and statistical requirements? Predictions or discriminations might be made only for individuals falling in nonoverlapping parts of the distributions; additional information could be sought for undecided cases. Trehub & Scherer (192), Hedberg & Baxter (77), and MacMillan (118) make similar suggestions in other contexts.

Among the tests of brain damage for which disappointing results were reported are the Dörken-Kral Rorschach signs (149), the Shipley-Hartford Retreat Scale, Weigl-Goldstein-Scheerer test, and Wechsler Memory scale (138). Some new tests, a visual-verbal concept formation test (51, 165), a proverbs test (56), a symbol arrangement test (132), a word-learning test (198), and block-sorting tests (5, 79) have promise not only for discrimination of organics from normals but also for differential diagnosis of organics vs. schizophrenics and mental defectives. A widespread error in reporting is illustrated by the proverbs test, for which the "concrete" score is said to differentiate organics from schizophrenics at the .001 level (56). That value is the significance level of the difference between groups, not the expected proportion of errors in diagnosis. Another neglected point is that proportion of errors must be determined with a different sample from the one used to determine the cutting point.

Exigencies of review require that the majority of the year's studies on tests be reviewed in relation to standard instruments. Campbell (21) has a typology of tests, but here used will be the outmoded classification into tests of ability, projective tests, and objective personality tests. Cattell (26) and Scheier (153) use the term objective for nonfakable, hence usually

physiological measures, which are herein omitted. The more usual connotation of objective scoring applies here, corresponding mostly to what Cattell calls questionnaires. New instruments will be slighted except where bolstered with considerable research or where they make a topical contribution. *Ad hoc* tests and MMPI keys are constructed in excess, as if it were easy to dash off a valid test of any given trait. Formally issued new tests are judiciously reviewed in the *Journal of Consulting Psychology* and both English and foreign language test editions are reviewed in *Diagnostica*. Some new tests are objective-projective hybrids, a promising trend. Allen (3) published an able text on personality assessment covering a wide range of tests.

TESTS OF ABILITY

The most frequently studied ability test is the Wechsler-Bellevue, particularly the current revision of the original Form I, the Wechsler Adult Intelligence Scale (WAIS), and the form for children (WISC). Similarity in content of the several forms increases their usefulness, but they are not always equivalent (95). Institutionalized defectives were tested with the WAIS and the Wechsler-Bellevue Form II; performance IQs correlated .8, verbal IQs .6 [Light & Chambers (112)]. For a group of above average intelligence, verbal IQs on the WAIS correlated .7 with those on the Wechsler-Bellevue Form I [Karson, Pool & Freud (95)]. A similar r was found for the verbal parts of college entrance tests, with nonverbal parts showing lower r [Klugman (102)]. Correlations between performance IQs in the study of Karson, Pool & Freud (95) were so low as to suggest checking first their accuracy, and if that is confirmed, their generality.

McNemar (124) showed in terms of subtest reliabilities why attempts to interpret scatter pattern of the Wechsler often yield negative or inconsistent results. Factor analyses of the standardization samples of the WAIS, separately for four age groups, were used by Cohen (31, 32) to derive definitive answers to the question of how to get information additional to that supplied by the full IQ from the WAIS. A general factor runs through all subtests, thus justifying the full scale IQ; separation of verbal and performance IQs is not supported. Most important group factors are verbal comprehension (involving Information, Comprehension, Similarities, and Vocabulary subtests), perceptual organization (Block Design and Object Assembly), and memory (Arithmetic and Digit Span). Absence of the number factor, conspicuous in other factor analyses, is notable, as well as the fact that both tests recommended for the memory factor involve numbers. Whether number and memory factors have been confounded can only be established in an analysis going beyond the limits of the WAIS. The crucial test of factors revealed by statistical analysis is whether they predict differentially nontest criteria. Current studies neither confirm nor disaffirm Cohen's analysis definitively. Delinquent adolescents do relatively well on Object Assembly and Picture Arrangement [Blank (13)]; Block Design

significantly differentiates patients with brain injuries [Parker (138)].

The excellent series on multifactor tests which ran in the *Personnel and Guidance Journal* has been reprinted in convenient form (182). Super (180) summed up the series: the Differential Aptitude Tests can be used cautiously for educational guidance, the General Aptitude Test Battery similarly for occupational guidance, but both need further validation, whereas the Holzinger-Crowder, Segel-Raskin, Guilford-Zimmerman, and Flanagan batteries merit further research. French (60) reported reasonably invariant factor loadings for a group of tests intended to measure 12 well-established factors. Vineyard (195) found that when students were tested with the Differential Aptitude Tests as high school freshman, the cross-validation multiple correlation with their first-year college grades was .56 for boys, .53 for girls. Differential prediction of different course areas was much less accurate and suspiciously capricious; e.g., the best predictor of the boys' social science grades was the Space Relations subtest, and the best predictor of the girl's language area grades was the Numerical Ability subtest. With the same test Hall (71) constructed a discriminant function for predicting the occupational groups later entered by students tested in high school; most of the discrimination was carried by general intelligence, largely verbal. Wellman's (201) results suggested that factor tests added a bit to differential prediction of high school grades in different subjects. Michael (127) added a programmatic statement about factor analysis contributing to differential testing but quoted no data. Has the mountain of factor analysis brought forth a mouse of differential prediction? Since some differential validity was achieved in Air Force studies during the war, the issue is not closed.

Goodenough's Draw-a-Man test remains a hardy perennial, both as an intelligence and a personality test. It has possibilities as a quick IQ estimate for the aged, with the height of the figure about as good as the score in terms of correlation with other intellectual measures [Jones & Rich (86)]. Sehringer (158) did an extensive review and restandardization for German children six to ten years old. German children exceed American norms beginning at the ninth year with the excess concentrated in certain features. Value for intellectual appraisal is limited by large variance at each age level. Children in Lebanon and Egypt showed declining Goodenough IQ with increasing age except when educated in Western schools [Dennis (45)].

Recognition of a social need and a construct validation approach distinguish the work of Owens, Schumacher & Clark (136) on measurement of creativity in machine design. Their best subtests were specific to the machine design area; general ability tests do not distinguish creative from noncreative machine designers, but sociability and egocentrism seem to characterize the more creative. Multiple correlations between their tests and the criterion classification as creative or noncreative are about .4 or .5, possibly inflated by elimination of individuals hard to classify.

PROJECTIVE TESTS

Rorschach studies.—In Rorschach work, as in assessment generally, there are those who believe that the clinician should apply the same rules to every case and those who advocate letting the clinician's unconscious resonate to each case. For the latter group, blind analyses are used to establish validity of inference; Piotrowski (139) advocates blind analysis as the backbone of teaching Rorschach. His book is of interest for historical materials and a review of the several interpretations advocated for various determinants by different schools, as well as a thorough exposition of his own system which stresses "the most original and least replaceable part of Rorschach's contribution: the formal or structural features of the percepts" (139, p. 3). Validity studies are not reported. Klopfer (101) reported surprisingly good separation of cancer patients into fast- and slow-growing groups on the basis of blind Rorschach analysis. He interpreted results in terms of ego defensiveness, i.e., the less defensive patients have more reserves to fight their illness. (Would it not be more parsimonious to suppose that rapid malignancy causes defensiveness than the other way round?) Powers & Hamlin (141) showed that experienced Rorschachers can make valid judgments using just three responses to Card I plus age and sex; their clinicians appeared to use knowledge of personality theory, intuition, and Rorschach experience, with more emphasis on content than on scoring categories. Mintz (129) found that personal problems contributed somewhat to diagnostic errors of clinicians but were not a major source of variance. Moss (130) discovered a few untrained schizophrenics with remarkable insight into the Rorschach protocols of other patients but also some tendency to project their own dynamics.

Armitage & Pearl (7) found that their psychologists using Rorschach psychograms or protocols, or both, could not distinguish among paranoid schizophrenics, unclassified schizophrenics, neurotics, and patients with character disorders. No factor or combination of factors was good enough for individual diagnosis, but the judgment of psychotic or nonpsychotic was made at a better-than-chance rate. Institutionalized idiopathic epileptic children could not be distinguished from a matched control with the Rorschach [Shaw & Cruickshank (162)]; negative relation between anatomy content and hostile acting out was not confirmed [Wolf (205)]; suicidal patients cannot be differentiated from nonsuicidal depressives [Schachter (151)]. Powers & Hamlin (142) analyzed what Rapaport called deviant verbalizations into four types, those showing intellectual disorganization, which differentiated schizophrenics from normals and neurotics; those showing deviant content; those showing inappropriate increase or loss of distance, both of which differentiated normal from patient groups; and those showing affectivity, which discriminated in an unexpected direction (i.e., schizophrenics showed fewer such responses). Productivity on the Rorschach correlates .4 or better with various intelligence tests, Wysocki (206) found. Sommer (171), however, using a psychiatric population, found that

IQ correlated .29 with productivity but .37 with number of human movement responses. More creative college students do not show more movement responses on the Levy Movement Blots [Griffin (69)]. Neither Clark (30) nor Sopchak (173) could improve prediction of college grades using the multiple choice group Rorschach. The fact that intelligence is about as predictable as anything from the Rorschach is striking. Surely no one will use the Rorschach to measure intelligence; but probably an independent estimate of intelligence is of value in interpreting the Rorschach.

A number of studies have added to the technical precision of Rorschach scoring. Norms for children aged six through eight have been added; a trial blot is not needed at those ages [Setze *et al.* (159)]. Some determinants, particularly shading, are more likely to be elicited with probing than without a thorough and pressing inquiry [Klingensmith (100)]. A scale for degree of popularity of response has been provided by Griffin (68). De Renzi, Isotti & Saraval (47) listed large details chosen by at least one-twelfth of their 300 normal Italian adults. Administration of several sets of blots showed that there are systematic changes in response upon such experience (57); perhaps more anxious subjects change more (111).

Rader (143) combined measures of aggressive impulses and measures of control in several ways; best prediction of overt aggression was obtained by multiple regression rather than by the several ratios that he tried. Standard Rorschach ratios should be subjected to just that kind of empirical-theoretical analysis. A similar problem was approached in a different way by Sommer & Sommer (172). Patients who had given aggressive and explosive color responses were compared with those having a similar number of color responses, none of them aggressive or explosive in content. Case history record of physical assaultiveness was significantly positively associated with aggressive color responses, but the association was not close enough for individual prediction. Sigurtà (166) gave the Rorschach in counterbalanced order with achromatic and hyperchromatic reproductions. The achromatic series elicited responses similar to the original blots, but the hyperchromatic series tapped a more superficial emotional level. Sigurtà concluded that shading is more important than color in eliciting emotion. Levy & Kurz (111), using standard and achromatic blots, concluded that the more anxious the Ss, the more important color was.

Several studies have correlated measures of postulated traits using the Rorschach with measures of the same traits using other instruments. Mandler & Parnes (120) discovered that frequency and idiosyncrasy of response were similar whether one used Rorschach cards, nonsense syllables, adjectives, or drawings; they ask, why not use the "more mundane stimulus materials?" Shipman (164), using a group form of the Rorschach and a number of other tests, failed to find appreciable generality to measures of "scope" and "differentiation." Palmer (137) sought relations between Rorschach's experience balance and patterns on the Rosenzweig Picture-

Frustration Study; results were suggestive only. Extratensive Ss perceived embedded figures faster and had higher cognitive complexity scores than introverted Ss, but that was not what Bieri & Messerly (11) predicted. Very little generality to the concept of constriction was found using the Rorschach, Thematic Apperception Test (TAT), Bender-Gestalt, semantic differential, an adaptation of the Allport-Vernon Study of Values, and peer ratings [Wohl (204)]. Shatin (161), on the other hand, used six indices of constriction from the Rorschach and seven from the TAT, correlating them separately. Most of the correlations were in the predicted direction and half significantly so; the best measure seemed to be " $M + \text{sum } C$," exactly the measure Rorschach himself proposed! Wohl's study did not use this measure but found a low, significant correlation between Rorschach and TAT measures of constriction. Studies of the foregoing type can profit from the shrewdest clinical intuition available and also from empirical experimentation with alternative measures of the constructs investigated.

Thematic apperception studies.—Three homogeneous groups of boys, very aggressive in socially unacceptable ways, very aggressive in socially acceptable ways, and passive, were selected by teacher nomination from an all-boy high school. In studying these groups, Jensen (84) distinguished projective from behavior-sample aspects of their performance on the TAT. While there was essentially no relationship between aggression in fantasy and in overt behavior, there was a highly significant direct relation between overt behavior and socially disapproved overt aggression in the TAT response viewed as a behavior sample, e.g., tabooed sex ideas and language. Socially unacceptable aggression was correlated with absence of fantasy themes of punishment and of defense against aggression.

Finally, an observation that is made too seldom in studies such as this, is the fact that, even though we were here dealing with groups differing in the extreme, the TAT variables discriminated very poorly for practical purposes. . . . That the TAT can be used for individual prediction of overt behavior is thus seriously questioned by these results, especially as regards the use of fantasy 'needs' and 'presses' (84, pp. 11-12).

Similarly, the delinquents of Lyle & Gilchrist (117) did not differ from their nondelinquents in the number of antisocial TAT themes, but the nondelinquents achieved more psychological distance from antisocial impulses by means of denial of reality, inhibition due to guilt, and rationalization. Hostility manifest by TAT heroes did not distinguish hospitalized schizophrenics with a history of acting out hostility from similar patients without an acting-out history, but the schizophrenics as a group displayed less fantasy hostility than hospitalized neurotics [Scodel & Lipetz (156)]. Resistance to black-out in a human centrifuge is related to the aggressiveness or dependence of one's TAT heroes (167). Using a sociometric measure of overt aggression and TAT-type pictures to elicit fantasy aggression, Lesser

(109) found that while overt aggression was only slightly related to score for fantasy aggression and (negatively) to number of stories in which anxiety prevented aggression, it correlated .5 with the ratio of the two scores. (Why a ratio rather than linear regression?) He also found a correlation of .4 between overt and fantasy aggression for boys whose mothers encouraged aggression and a corresponding negative relation for boys whose mothers discouraged it (108). Dependent behavior exhibited on the TAT does not seem to be related to overt dependent behavior but is somewhat related to interview ratings of conflict over dependency [Fitzgerald (58)].

Use of the TAT to measure need for achievement (*n Ach*) was apparently proposed without regard for a Walter Mitty effect; that fantasy achievement may sometimes substitute for rather than register overt achievement behavior is now becoming evident. Vogel, Baker & Lazarus (196) found a slight negative relation between performance on a laboratory test and TAT-measured *n Ach*. Their results emphasized the specificity of the achievement motive; motives which Ss demonstrate in one context may not be engaged in another. Lazarus *et al.* (106) discovered that high school students displayed more *n Ach* than college lowerclassmen, who exceeded upperclassmen. These and other results led the above authors to question the assumptions underlying the method of McClelland and his co-workers. Other studies finding no relation between *n Ach* and actual achievement were reported by Alper (4) and Krumboltz & Farquhar (104). Birney (12) found a correlation if students administered tests but no correlation if faculty administered them. There is a relation between *n Ach* and vocational aspiration [Minor & Neel (128)].

Krumboltz (103), reviewing the literature, found no measure of achievement motive which a counselor could use with confidence. Inventories of Fricke and of Brown & Holtzman are of some value in the academic situation; McClelland's use of the TAT and French's Test of Insight are the best projective measures but they have not been validated for individual prediction. Users of the French and McClelland measures of *n Ach* must be careful, as both tests are sensitive to circumstances of administration. The authors take this fact as evidence of construct validity, but for practical purposes it is a weakness (103). The same problem arises in validating tests of anxiety and is discussed by Malmö (121), among others. French & Thomas (59) studied the relation of French's measure of *n Ach* to a laboratory problem situation. As in many other studies in this area, interaction effects were more impressive than main effects, which augurs poorly for straightforward measurement.

TAT cards differ in the number of emotional words they elicit; clinicians in training tend to use the more favorable ones (194). A set of pictures can be designed with first thought to productivity or to areas covered; Sherwood (163), in designing a set of cards to be used with an African

people, tended toward area coverage. Verbal descriptions of TAT cards, without actual presentation of the cards, elicit stories not too different from those using cards [Lebo & Harrigan (107)]. Friedman (61) presented a Q-sort deck to use in describing TAT heroes. Mandler, Lindzey & Crouch (119) found formal TAT variables more promising than content variables as measures of anxiety.

Furuya (62) found no support for Bellak's belief that the animal pictures of the Children's Apperceptive Test elicited greater productivity from children than a corresponding set of human pictures. Boulanger-Balleyguier (14) studied the same test in detail, listing popular responses for each card, sex differences, and age trends. Children tend to cast most of the animals in a maternal role, with the exception of the lion. Buss & Durkee (18) studied the association of animals with family figures in adult patients and normals. Their results, as well as those of Boulanger-Balleyguier, lead one to question some of the accepted Rorschach content interpretations.

Other projective techniques.—Hammer (72) and Caligor (20) presented in books their methods of using projective drawings clinically. A variation of the technique has the patient draw a person as an idiot would; uninhibited drawings result [Ponzo(140)]. Swensen (183) reviewed the literature on drawing a person in terms of Machover's system of interpretation. He found only modest confirmation (group trends) of some of her hypotheses, and no evidence to indicate that the hypothesized signs were of value for individual diagnosis; admittedly, many of the studies were not adequately designed for the purpose. More recent studies have not altered his conclusions (17, 145).

Attempts to objectify projective techniques have taken the form of systems of codifying responses or substitution of multiple choice for free responses; Wallon & Webb (197) worked out an original alternative for the Rosenzweig Picture-Frustration (P-F) Study. They gave the P-F Study first as a projective test. They then asked S to choose on a multiple choice form the answer which most resembled his response on the projective test. Another group was given the multiple choice form only, and a third group was asked to take the multiple choice form putting themselves in a favorable light. The proportion of extrapunitive responses went down sharply, proceeding from the projective form, through the projective-objective, objective, and "best" answer. Silverstein (170) also found that Ss gave fewer extrapunitive responses under instructions to make a good impression, and 95 per cent of his Ss gave extrapunitive responses when asked to make the worst impression. Captions of the P-F Study presented without the pictures elicited responses comparable to those with the conventional form [Schwartz (155)]. Lesser (110) confirmed a positive relation between extrapunitiveness as measured by the children's form of the P-F Study and anti-Semitism. However, extrapunitiveness was unrelated to ratings for aggression among delinquents [Norman & Kleinfeld (134)]. Graine (67) found

anomalies in the Group Conformity Ratings of the P-F Study, including a correlation with the Edwards Personal Preference Schedule which went the wrong way.

The Bender-Gestalt test is generally used now with a modification of the Pascal-Suttell scoring, often as a recall rather than as a copying test (135, 177). Sometimes it provides significant differences between psychotic and nonpsychotic patient groups (177), sometimes not (135, 185). It seems to differentiate organics from other patients (191) and delinquents from nondelinquents (207). The validity of the Szondi test has not improved with age (83).

Two manuals describe the administration, scoring, interpretation, and research background of the Kahn Test of Symbol Arrangement (91, 92), and additional studies continue to suggest its worth (93, 132). Part of the administration and recording and all of the objective scores appear to be unused in the discriminations which have been established by research and which depend on level of abstraction, as shown by the number element and symbol pattern. Choice of materials, scoring, interpretations, and diagnostic rules are based on intuition, theory, and data; clearer exposition as to which aspects of test construction were guided by theory and which by data would be desirable. The material is attractive and usable over a wide range of age and ability.

A test consisting of seven cartoons depicting children, teachers, and parents has been in use since 1950 at the Bank Street College of Education [Shapiro, Biber & Minuchin (160)]. The S tells what he sees in each cartoon and develops the implications. Reliably scored aspects of the test have been shown to be related to teaching criteria, and the test has also proved valuable when used globally in counseling. An interesting finding is that too close identification with cartoon characters, particularly children, is not a favorable sign. "Nonidentification in the testing situation, rather than implying distance or a coldly intellectualized approach, is related to an appropriately warm responsiveness in the behavioral situation" (160, p. 180).

Various ways of using word association (76, 78) and sentence completion (10, 123, 179) as the basis for more or less projective tests continue to show promise. Getzels & Walsh (65) used paired projective and objective forms of the sentence completion test. The stems were identical but used names in the projective test, which was presented as a speed test, and used "I" in the direct version. Replies were scored for positive or negative affect. Jonsson (88) made a detailed study of questionnaire and interview methods, also comparing direct with projective items. His direct items began "Do you . . ." or the like, his projective items, "Do you think that people you meet . . ." Both studies found that people have more socially desirable or positive responses under direct self-reference. Getzels & Walsh found this tendency to increase with age between 8 and 13 years, which they inter-

puted as evidence for socialization. Jonsson, using young adults, found validity coefficients were modest for direct questions, negligible for projective ones. Getzels & Walsh used a subtler model of projection than Jonsson, since they inferred from a disguised test what Jonsson asked directly. Murstein (133) discussed models of projection and methodological difficulties in their measurement; but his "classical projection" made no provision for reversal of affect, as Freud did. An ingenious technique for assessing the degree of a child's positive and negative feelings toward members of his family was presented by Anthony & Bene (6).

OBJECTIVE TESTS

Predominant interest in relation to objectively scored tests lies in the problem of response bias. Four major forms are tendencies to acquiesce, usually to clichés; to deny, usually symptoms; to take extreme positions in degree-of-agreement items; and to describe oneself in socially desirable terms. Authoritarianism, as in the California F Scale, is typically confounded with acquiescence and extremity. Neuroticism and anxiety are typically confounded with denial and social desirability. Acquiescence and denial are intrinsically incompatible response sets but both are at times confounded with social desirability, and they tend to be related orthogonally rather than negatively.

The literature on social desirability has been summarized by Edwards (55). The major finding, that there is a high rectilinear relation between judged social desirability value of an item and tendency of people to attribute it to themselves, appears more remarkable than Edwards allows. Are there no virtues too exalted to claim? Are they not socially desirable? Or just not items? "Social desirability" is scarcely a psychological concept; Edwards did not follow Rosen's (146) suggestion to distinguish personal values from perceived social values. Edwards noted that the MMPI K scale measured social desirability except for five items keyed the wrong way. But the K scale was intended as a suppressor variable and was shown to function more effectively with the additional items. The neat but ultra-empirical approach to social desirability has neither penetrated the psychology of the phenomenon nor added to any nontest predictions.

Gage, Leavitt & Stone (64) treated response bias as itself a psychological problem. Beginning with the asymmetrical relation between acquiescence and authoritarianism as measured by the F Scale and its reversals, they concluded that acquiescence was itself an aspect of authoritarianism; therefore, the F Scale was properly constructed so that authoritarian and acquiescent tendencies always operate in the same direction. However, Christie, Havel & Seidenberg (28) doubted the conclusion of Gage, Leavitt & Stone, partly because previous reversals of the F Scale, on which it depended, did not follow the theory underlying the test. The reversed F Scale of Christie, Havel & Seidenberg will probably remain the definitive one. It

correlated about .4 with the original, indicating that content accounts for more variance than acquiescence; however, the value would be about .2, similar to previous results, had they not corrected for attenuation. In sophisticated college groups Christie, Havel & Seidenberg found no evidence of response bias, but among freshmen there was an acquiescent tendency. Ss highest on the F Scale showed a tendency toward extreme responses in both directions on the reversed F; those somewhat high showed a tendency toward acquiescence; while those lowest on F showed greatest tendency toward ideological consistency.

There are many glimpses into a complex network of interrelationships which remains to be definitively described. Whatever the F Scale measures is related to intelligence and educational level most consistently (33, 43, 131) and also to age, tending particularly to decrease during the college years (28, 148). Cohn's (33) term, intellectual sophistication, seems a particularly happy one. Christie & Cook (27) found the same trends in earlier studies with the F Scale. There are continued indications that the F Scale is not measuring a unitary function [Livson & Nichols (113)].

The F Scale, using third person items, can be divided into subscales measuring conformity and ego defense. The latter is significantly related to an MMPI scale using first person items to measure ego defense [Katz, McClintock & Sarnoff (97)]. In building a measure of the nonpolitical aspects of authoritarianism, called heteronymy, Jones (87) used forced choice between pairs of alternatives to eliminate acquiescence and extremity biases. His Pensacola Z Scale, composed entirely of second person items, correlated .4 with the F Scale. Four subscales, intended to measure aspects of heteronymy, are dependency, rigidity, anxiety, and hostility. Since there is only one positive correlation between subscales, one may well question whether these subscales are reasonably conceived as aspects of a single trait. When asked to make "best" answers, scores on dependence and rigidity increased, while scores on anxiety and hostility decreased. Kaess & Witryol (90), also using the Z Scale, confirmed that favorable faking resulted in a slight increase in authoritarianism and found a marked decrease when students were asked to fake unfavorable scores. Thus, while the forced-choice form eliminates acquiescence and extremity biases, the social desirability bias must still be contended with. Whether the authoritarian direction is perceived as socially approved is a function, among other things, of the degree of authoritarianism of the individual [Christie & Cook (27)], a more complicated state of affairs than Edwards allows for.

Hanley (73) selected MMPI items for which social desirability was minimally correlated with probability of endorsement ($r = .3$). This measure of defensiveness (social desirability) had correlations with clinical scales similar to those of the K scale, negative with obvious keys and positive with subtle keys. Further selection left a set of items which could be keyed for acquiescence and for defensiveness with almost no correlation

between the two keys. Though defensiveness contributed more variance to clinical scales, acquiescence also appeared to make an appreciable contribution. Buss & Durkee (19) used rationalizations and idiomatic expressions to reduce defensiveness in a new hostility inventory, and they also found an r of only .3 between social desirability and probability of endorsement.

The Edwards Personal Preference Schedule, which uses forced choice between alternatives paired for equal social desirability, is only moderately and perhaps not inappropriately influenced by social desirability, according to Edwards (55), Silverman (168), and Kelleher (98). However, the r of .3 between some of the Edwards scales and social desirability is about what Buss & Durkee achieved by careful phrasing of items without forced choice. That the statistical properties of items administered independently change when they are assembled in forced-choice format is algebraically obvious but apparently has only just been noticed as an empirical and dynamically plausible fact [Spector (174), Corah *et al.* (37)]. Corah *et al.* presented evidence for appreciable social desirability bias in some of the Edwards scales.

The Heineman forced-choice form of the Taylor MAS is unrelated to the MMPI K scale, in contrast to the MAS, and is significantly related to measures of skin conductance under threat of shock, which the MAS is not [Silverman (169)]. Edwards' (55) suggestion that the relation between the MAS and various learning tasks may be the result of a desire to make a favorable impression in both is confirmed by Martin (122), who found a conspicuous "try" factor in a study of anxiety. [The degree of involvement or motivation in laboratory tasks was also identified in achievement measurement by Vogel, Baker & Lazarus (196).] Personality, anxiety, and muscle tension have been shown to be related to response stereotypy, with interaction effects statistically significant [Kuethe & Eriksen (105)]. Even in an achievement test, positive statements tend to be answered "true" and negative statements "false" [Whipple (203)]. The tetrad form of forced-choice items does not appear to work well [Bass (8); Spector (174)].

The MMPI continues to generate a wide variety of research. A configurational analysis of scales distinguished neurotics from schizophrenics "at a very high level of confidence" but with from 10 per cent to 20 per cent misses [Taulbee & Sisson (187)]. A scale for sexual deviation didn't hold up (81, 199); an ego-strength scale differentiated students from patients (184) but not psychotics from neurotics (186). The index F-K correlated .6 with number of children among job applicants (50); that represents one of the highest correlations for the year between a test score and a nontest variable. An MMPI pattern predictive of academic achievement with the Mf scale used as a suppressor variable has been noted (49). This study was one of several in which the records which gave rise to the hypothesis may have been included in those used to test it. Other studies where this doubt was not resolved have been omitted. Hathaway & Briggs

(75) presented norms for some of the new scales. More factor analyses resulted in more suggestions for scale revisions [Comrey (34, 35, 36)].

Karson & Pool (94) intercorrelated the scales of the 16 Personality Factors Test with those of the MMPI. Correlations between Forms A and B of the former test are mostly too low for them to be considered equivalent; factor B is not a good measure of intelligence; several factors correlate significantly with the L, F, and K scales, suggesting need for correction for test-taking attitudes; and no judge could predict better than chance the direction of intertest correlations, which casts doubt on Cattell's interpretations of his factors. On the other hand, the Cattell test appears to cover about the same ground as the MMPI though it is a shorter test. In a similar study Allen (2) compared the Edwards Personal Preference Schedule with the MMPI. The Edwards test seems to have less overlap with the MMPI than does the Cattell test, no doubt because of better control over response bias.

A test of impulse expression has come out of the Vassar study of Sanford, Webster & Freedman (148). Despite a convincing psychological presentation, the test is probably over-weighted with response bias because of the yes-no item form. The authors found a correlation of .58 with their own measure of defensiveness [Webster (200)]. The chief importance of the Vassar project is its sensitive and detailed portrayal of changes in test and other behavior during the college years, facts which must be taken into account by future test constructors. Other new tests suspect of undue response bias are those of Macmillan (118) and Schaefer & Bell (152). Demonstration of appreciable validity in no way cancels demonstration of distortion of measurement by response bias. For individual diagnosis one might be able to ignore such distortion, but where are the nonability tests with proven worth for such purposes? In studying group differences the effects sought may be of the same order of magnitude as the effect of bias, Dunsdon & Roberts (54) observed. Thus greater freedom from distortion is required for research than for diagnostic use. The prevalent opinion, that large errors are tolerable in research but not in clinical use, is correct only for random errors. Brogden (15) has been concerned with a similar point.

One Vassar finding was that there is more than one masculinity-femininity dimension (148). Reed (144) and Tolor (190) similarly found little in common between different measures of masculinity-femininity. Brown (16) presented findings with his "It Scale for Children," which reveals sex-role identification in a setting projective in feeling but objective in scoring.

COMMENTS

Use of tests for cross-cultural comparisons appears to be an increasing trend. Dennis (46) devised a test simply asking the use of common objects, which proved rewarding as a method of cross-cultural study. Doob (48) reported on use of various test items with nonliterate peoples. Sherwood

(163) constructed a set of TAT pictures for Africans. Cross-cultural research is promised early publication in the *Journal of Social Psychology*. Kataguchi (96) described the use of the Rorschach test in Japan. A myokinetic technique with which much research has been done in South America (24) will soon be introduced in the United States.

By sheer numbers, American articles set the year's trends, but first-rate research is being done in many countries (e.g., 14, 62, 158, 166). Some articles published in French and German journals would be rejected by many American editors for their murky theorizing, desultory substantiation by case histories, citation of averages without variances, etc. If seen through European eyes, would American journals reveal any pervasive weaknesses? The reviewer suspects so. American editors in many instances appear to have a Baconian view of science—data, however poor, given precedence over ideas, however good. Some editors encourage, or at least tolerate, several brief reports from an author on the same subject in a short time. Report on a small study may be split into articles sent to several journals. Such fragmentation of data makes the task of literature search and comprehension needlessly difficult.

Some methodological lapses have become unaccountably epidemic, probably occurring more often than their correct alternatives. They include (a) the floating level of significance, (b) discarding all but extreme cases, and (c) dichotomization of quantitative measures. As to (a), many authors appear to believe that to every difference or r there corresponds a level of significance. Thus one article reported levels of significance ranging from .01 to .99 (135). Holt (80) illustrated a desirable countertrend, indicating by typeface whether each r was significant at the .01 level, the .05 level, or not significant. The level of significance must be selected in advance and not altered after looking at the data. Regarding (b), it should be noted that the study of extreme groups, while valuable in exploratory and adjunct studies, can often be shown not to give the same results as study of the entire range of the sample (29); their study may lead to exaggeration of small relationships or obscuring of significant ones. With respect to (c), Gundlach (70) has commented on the oddity of discarding quantitative information and using only its dichotomized residue, usually above or below the median. Rarely has the procedure been given specific justification in terms of problem content. Probably underlying all three errors is the chic but fallacious use of statistical significance in place of correlation, per cent overlap, etc., as measures of degree of relationship, a practice to which Dressel (52) entered objection. Ironically, random sampling is a rarity. Complex computations may be undertaken with a dozen or two cases, yielding more coefficients than there were students in the author's class.

To date, the only tests which meet standards for individual prediction are those of general ability. In view of the limited predictability of human behavior and achievement by any current means, the moral burden of the

assessor is great. If he permits his judgment latitude to take into account all the facts he may overvalue winsomeness, while if he follows an actuarial rule strictly assessment becomes a scientific version of prejudice. Intuitive use of projective and other materials continues to provide occasional remarkable insights. The strictly actuarial approach has few triumphs this year. The moral dilemma can be almost resolved by the solution which also has most scientific merit: construction of valid measures of maximally relevant traits. Utilization of such quantitative information will probably best be handled by empirical regression equations. Single signs from projective tests compare unfavorably as predictors with combinations of signs or scores; but predilection for combination by ratio and by configural scoring is not based on demonstrated superiority to regression equations. Interaction effects are significant, often more so than main effects, but the capture of scorable interactions remains an unrealized program.

Studies of objective tests give promise of penetrating further into the psychology of test behavior than heretofore. A conceptualization which includes much of the year's work in this area as instances is the following: The major variable in personality test responses is intellectual stereotypy at one extreme and intellectual sophistication at the other. Individuals at different points on the continuum differ not only in their expressed beliefs and self-concepts but also in their modes of verbalization. Degree of stereotypy is difficult to measure because it may be manifest in overlapping or alternative ways. Ideological authoritarianism is itself a tendency toward stereotyped thinking; so is response bias, whether of acquiescence or, more commonly, favorable self-portrayal. In most tests the relations between the several forms of stereotypy are built in. Proliferation of tests of high-sounding psychological constructs in disregard of response bias is conspicuous waste of research.

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PSYCHOTHERAPY^{1,2}

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Just before the writing of this review, a three-day conference, "Conference on Research in Psychotherapy," sponsored by the Division of Clinical Psychology of the American Psychological Association, took place. Thirty participants were present, representing approximately seventeen psychotherapy research projects. While the contents of this conference will not be covered (since a book-length summary of it will appear within the next year), it provided an excellent up-to-date overview of psychotherapy research. Some of the ideas in the present review were derived from this conference.

The main first impression one gets from the conference and from reviewing the literature and searching for trends is of bewildering variety: every one is going his merry way. The contributions during the year 1957-58 are a museum of method and philosophy of science. If we classify the types of research according to Helen Sargent's scheme [Wallerstein *et al.* (120)], they range all the way from exploratory to rigidly experimental. Not only do they differ in method but in aim, in the segment of psychotherapy focussed upon, and even in whether they focus on psychotherapy academically as a facet of personality research, or whether they focus on it clinically with the aim of learning how to help patients get better.

Not only does variety in research studies make trends difficult to see clearly but the field itself is one in which not much change can be expected in a period as short as a year. One might be better able to discern definite trends in a five- or ten-year period. Finally, a summary of the work of the past year inevitably focuses our attention on what is novel, and it becomes too easy to mistake novelty for progress. Older contributions often have more to teach us than the newer ones. Without a doubt, most of what we know about psychotherapy was presented in the work of Freud, and we have yet to learn how best to use the fruit of his excellent observations.

Despite the diversity, on close inspection trends are apparent. This year, as in recent years, the methods of research and the concepts applied to research have become more sophisticated and appropriate to the problems in the field. Better tools are available. A number of new sys-

¹ The survey of the literature in this review was completed in May, 1958. It covers the period from April, 1957, to April, 1958.

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tems have been developed for content analysis of psychotherapeutic interviews. Some of these this year appear promising, particularly those of Strupp (103), Rogers (88), and especially the forthcoming work of Leary & Gill (59) who have succeeded against great difficulties in constructing a reliable coding system incorporating psychoanalytic concepts.

Fortunately there have been recently fewer of the simplistic type of outcome studies in which the end point of the study is to present percentages of patients who have improved or have not improved, broken down according to diagnostic labels.

Again, as last year, there are more group projects which represent the work of several individuals or even large groups. Many of these are just getting under way or are in the middle of their endeavor. We should see a considerable increase in our knowledge of psychotherapy when these projects come to fruition in a few years. A listing of their locale will give an idea of the size of our national research investment in psychotherapy. The following includes the principal ones known to the author: Los Angeles, Calif.; Chicago, Ill.; Ann Arbor, Mich.; Bethesda, Md.; New Haven, Conn.; Madison, Wis.; Topeka, Kan.; Baltimore, Md.; Chapel Hill, N.C.; Oakland, Calif.; Washington, D.C.; Urbana, Ill.; University Park, Pa.; Portland, Ore.

This review will evaluate as well as summarize the literature. No review in a field so broad and prolific as psychotherapy could be complete. However, an attempt has been made at complete coverage of the quasi-experimental type of research contributions. Theoretical contributions summarized represent a selection from many which seemed repetitious or unimportant.

An outline for presenting the work of the year, similar to Snyder's review of 1956-57, will be used. The principal distinctions are between the research contributions and the theoretical ones. The dichotomy between the two is not entirely accurate but is conveniently applicable to the literature. The divisions of the research contributions, however, are further subdivided on the basis of whether the principal focus of the contribution is upon variables in the therapist, treatment, patient, or situation. This outline will be followed in discussing the work of the year:

- I. Research contributions (of a quasi-experimental type)
 - A. Studies of the therapist
 - B. Studies of the treatment
 - C. Studies of the patient
 - D. Study of the patient's life situation
 - E. Studies of physiological changes accompanying psychotherapy
 - F. Psychotherapy research methodology
- II. Theoretical contributions
 - A. The theory of psychotherapy
 - B. The theory of psychoanalysis
- III. Special applications and techniques
 - A. New techniques of psychotherapy

- B. Psychotherapy combined with drugs
- C. Treatment of children and adolescents
- D. Group psychotherapy
- E. Teaching and supervision of psychotherapy and psychoanalysis

IV. Reviews

V. Summary of trends

RESEARCH CONTRIBUTIONS (OF A QUASI-EXPERIMENTAL TYPE)

Studies of the therapist.—There have been few experimental studies of the therapist in comparison with the number of studies of the patient or the treatment. One of the best studies of the therapist is by Strupp (105). It is an inquiry into the therapist's technique and how this is influenced by his background and personality. A group of 237 therapists of varied backgrounds watched a film entitled "The Clinical Picture of Claustrophobia." At various points in the film a 30-second blank space was inserted with the question, "What would you do?" Among Strupp's many interesting conclusions was one that this group of therapists could be divided into two "ideal type" groups: Group I are more "tolerant, humane, permissive, democratic and therapeutic"; Group II are more "directive, disciplinary, moralistic and harsh." Strupp speculates that therapy is most effective when (a) the therapist can be warm and empathic and (b) he can use "appropriate technical devices." Nothing much can happen unless (a) is present, and (a) and (b) achieve best results.

The reviewer believes that films used in this fashion provide a standard experimental situation to which all therapists can respond, and that this is a useful procedure, even though it involves a compromise with the need for a "real" situation. The 30-second interval provided by Strupp for the therapists to respond seems unduly short. The instructions given the therapists required them to deal with the patient diagnostically or therapeutically, as they saw fit. This seems like an unnecessary cause of variation in their responses. It would have been better to have made the instruction more uniform, so that all the therapists' responses could be viewed with the assumption that they were intending to treat the patient, not dispose of him. Furthermore, since the audience was told that the therapist in the film had agreed to see the patient only once, the therapists in the audience were precluded from doing most of the things that one would ordinarily do in a treatment situation.

Perhaps a reader could gain a better understanding of the therapists' responses if independent assessment of the patient were provided. To have provided this would have come closer to ordinary clinical practice. In addition, it was noted that understanding of the nature of the patient appears to have a direct relationship to the level of experience of the group of therapists, with both the more experienced psychiatrists and therapists tending to be those with the more "negative" attitude toward the patient. Further analysis of

these findings showed that the attitude of the therapists and psychiatrists toward the patient was influenced by the patient's actual diagnosis. The question arises, would this relationship between attitude and experience have held with a patient of another type? This is obviously a good question for further research. It is true that almost no one among the group of therapists expected basic personality change in the patient, and the more experienced therapists were therefore with the majority, since the majority regarded the prognosis as unfavorable. Also, possibly, the more experienced therapists were more able to give an honest reaction, as compared with the inexperienced therapists who perhaps had to keep themselves reacting in terms of their conception of the model therapist—for whom every patient should have an excellent prognosis.

Many experienced therapists have recorded their impressions about the personality of the psychotherapist, psychoanalyst, psychiatrist, and clinical psychologist. The consensus of these observers has been that there is a considerable overlap in the personality qualities requisite for each of these professions. Rubinstein & Lorr (93), as part of a research project evaluating changes resulting from psychotherapy, had 67 psychotherapists in Veterans Administration mental hygiene clinics rate themselves on "The Multi-dimensional Scale for Rating Psychiatric Patients" (92). At the same time two of their peers who knew them well judged each of the psychotherapists on the same scale. There were no significant differences among the psychologists, social workers, and psychiatrists on the six main factors derived from the scale. There were, however, expected significant differences on the factors between these therapists and a sample of patients.

A study by Lakin & Lebovits (58) is somewhat similar in design to the foregoing Strupp study. Lakin & Lebovits inquire into the way therapists of different orientations understand a patient about whom only a small amount of identifying information is given. The aim is to determine how different therapists are "prepared" to see a patient. Seventeen therapists of different "schools", each supplied with a seven-line factual description, "free-associated" to the question: "As a psychotherapist how would you think about this person?" The selective emphasis in the therapists of the three orientations was discussed. The experimental situation seems unnecessarily unnatural. It makes diagnosis too much of a free association experiment, rather than conceptualization based on adequate information. By making it so much of a free association experiment, one maximizes the chances for obtaining in undiluted form the personality and the professional orientation of the therapist, rather than a more accurate evaluation of the patient. It would be pertinent to see how much convergence would be imposed by additional information about the patient.

Studies of the treatment.—The studies to be summarized here can be roughly dichotomized into those with principal concern as to how changes took place, therefore focussing on the interchange between patient and

therapist (*i.e.*, the process), and those that focus on the end point, to answer the question of what change took place (*i.e.*, the outcome). The outcome studies summarized in this section are those studies which compare outcomes in different kinds of treatment.

This year, as in other recent years, a number of systems for analyzing the process of treatment have been developed. A very competent system for codifying what the therapist says in psychotherapy is reported by Strupp (103). He reports a well-conceived and executed system. Each intervention of the therapist is viewed simultaneously from five vantage points: (a) the kind of technique employed by the therapist (e.g., question, interpretive statements, authoritative opinions); (b) the amount of inference employed by the therapist in his comments to the patient; (c) the focus of the therapeutic interventions; (d) the degree of initiative assumed by the therapist; and (e) the warmth vs. coldness in the attitude of the therapist. Scales and codes are provided, so that observer reliability is good.

The limitations mentioned by Strupp in his discussion of the scoring method are worth summarizing, since they apply not only to his own but to many scoring systems in the literature. (a) This system applies only to what the therapist does. (b) It deals with that which falls within it in an atomistic manner. Special meanings falling outside of the code cannot be scored. (c) It takes considerable time to score each hour, approximately two to four hours. (This seems about right. It takes at least an hour to listen to an hour, and it is complex material, so that one cannot just go right through it. One should be suspicious of a system that is too easy.) (d) The system has nothing to say about whether an interpretation is right, or is timed properly. It is therefore necessary to have independent estimates of the correctness of interpretations. (e) It takes account only of the verbal material. (f) Each scoring unit is treated like each other unit. These limitations are fairly typical of most scoring systems. It therefore remains for a clinician to come along and apply what Sargent (94) calls "perspective" to make sense out of the material and decide which are the pervasive and crucial interventions.

The value of this system is illustrated by Strupp (104) by applying it to the published transcript of a case treated for nine sessions by L. R. Wolberg. It is demonstrated that the profiles derived from the scores are meaningfully related to an understanding of the psychotherapy and provide a quantitative characterization of the technique and the variations in it from session to session.

Strupp (102) also makes an excellent comparison by means of his scoring system of two completed treatments reported in the literature. The first is the one by Wolberg reported in the previous article, and the second is a report of a treatment by Carl Rogers. Strupp shows convincingly that one can compare treatments quantitatively and meaningfully by means of his system. He observes that researchers have paid too little attention to the therapist. Everyone working in this area nowadays notices this fact,

but too few studies have tried to do anything about it. Strupp's codification shows common factors in the therapist's verbal operations in the two types of treatment. The client-centered treatment focused mainly on reflections of feeling. The analytic therapist used more inferential techniques, and showed more initiative than the client-centered therapist. The client-centered therapist did not change much from session to session; the analytic therapist changed very significantly from session to session, especially in type of therapeutic activity, degree of inference, dynamic focus, and initiative.

Kerzman & Peek (50) tried to categorize some methods of communication in psychotherapy. Their purpose is to use the categorization as a check on what therapists are expressing in treatment and what they may be overlooking. The way the patient and therapist see each other and what they expect of each other can be determined with a system developed by Chance (17).

One of a series of studies of treatment done under the supervision of W. U. Snyder at the Pennsylvania State University is reported [Ashby *et al.* (4)]. It is a comparison of the nondirective type of treatment with a more "leading" type (i.e., directive and interpretive) to see whether one produces more resistance, dependency, etc., or whether patients with certain personalities relate themselves to a therapist better in one treatment than in another. A carefully-worked-out design is provided to answer these questions. Training was given in two treatment methods to ten therapists. Patients were assigned randomly to the therapists and to the two types of treatment. Each therapist had four patients, two in each type of treatment. The two types of treatment resulted in different effects on patients. Furthermore, characteristics of patients had slightly different consequences in each type of treatment, while therapists had different effects on patients, independent of the type of treatment. Finally, there was an interaction of the personality of the therapist with the type of treatment he used in terms of its effects on patients; for example, patients may be defensive in one type of treatment offered by a therapist, but his other patients treated by another method may not be defensive.

The authors report with unwarranted surprise a difficulty in keeping therapists to a particular technique when the therapist felt it was not in the patient's interest. The predominant preference of the group of therapists was for the "leading" type of treatment in comparison with the nondirective type. This may explain one of the findings: patients in the leading type of treatment tended to become more positive in their feelings toward treatment. The change in patients' attitudes may reflect a preference of the therapist for the particular method of treatment. It may reflect, as well, the fact that a therapist who uses the method he prefers does a better job. This suggests a difficulty in any design for psychotherapy research in which a therapist is asked to carry out a treatment procedure which he is not convinced has merit. Patients must often recognize when a therapist is not convinced

of the efficacy of a treatment procedure in which he is engaged. Interventions by the therapist in such a treatment may even be felt by the patient as lacking in sincerity. Therapists must also experience discomfort with a treatment procedure in which they do not believe. It would be interesting to know whether the same results would emerge with therapists of greater experience than the ones used in this experiment.

The word "leading" in connection with treatment seems an unhappy choice of term. The term is unusual and the method does not have as clear-cut a definition as the client-centered one.

A special asset of the procedure for classifying the responses of the patient to the therapist involved the playback of recordings, rather than the more usual reading of transcripts used in other researches. The authors carried out a type of research which is most difficult to handle experimentally, that is, a comparison of the results of two types of treatment. The main limitations of their design had to do with inexperience of therapists and the use of treatments in which the therapists may not entirely believe. A design that ought to be tried in the future would utilize expert therapists carrying out their preferred type of treatment and then having scoring schemes applied to these treatments from a variety of conceptual points of view.

Gliedman *et al.* (36) reported the latest in an interesting series of studies of placebo or "dummy pill" effects by the Henry Phipps Psychiatric Clinic group. This report compares the influence of placebos on outpatients who have had short-term psychotherapy, with the changes produced by the psychotherapy. It also provides a reanalysis of four previous placebo studies. Symptom reduction was found to be similar for the placebo and for short term psychotherapy. The authors speculated that placebo effects bring out influences which are common in all forms of psychotherapy. These common influences must be understood before contributions of specific psychotherapy techniques can be comprehended. In explaining the placebo effect, the authors postulated that a person's reaction to illness may interfere with the healthy aspects of his personality. Diminishing the "secondary reaction" may free healthy parts of the personality so that what appears to be symptomatic improvement may become more generally improved functioning.

The authors do not appear to be surprised that placebo effects, according to their interpretation, are as great as those coming from short term psychotherapy. They must believe, therefore, that placebo effects are large, substantial, and lasting. Actually, the results may be looked at in another way: short term therapy effects may be slight and, therefore, similar to placebo effects. This hypothesis cannot be ruled out by their design. The two standards for measuring change are incorporated in scales for measuring "ineffectiveness" and "discomfort." These may not be sufficiently comprehensive to cover changes produced by short term psychotherapy. Long-term follow-up results are not yet available on the placebo effects. It still seems a good assumption that, except for an occasional patient who has a strong

response to the placebo effect, most patients, particularly seriously ill ones, will get relatively little lasting value out of the placebo, somewhat more out of short-term therapy, and even more potentially out of long-term treatment.

A study is described by Worchel (125) on the effects of different cathartic techniques on the relief of hostility feelings. It was a typical psychology-class experiment with a control group and experimental group. The experimental group was given an "insult" during the administration of a test. The results in general confirm our knowledge that catharsis (here in the form of eight minutes during which the group is allowed to express criticism) can reduce feelings of hostility.

The last two studies in this section are more typical outcome or improvement studies. Ellis (25) compared percentages of improvement obtained by a single therapist employing orthodox analysis, psychoanalytically oriented psychotherapy, and "rational" psychotherapy, with matched groups of patients. He found that the last produces more improvement than the other two and he seems inclined to conclude that these are therefore "not the last word" in technique. Thorne (112) addressed himself to the old question whether a certain kind of therapy does any good; in this case it is "eclectically oriented psychotherapy." He picked out fifty patients who had been treatment failures by other kinds of therapy and found that his therapy produced improvement in many of them.

Studies of the patient.—A number of these studies can be grouped together. They are concerned with identifying qualities of patients that are associated with staying in treatment, improving in treatment, or returning once having left treatment. There are many such studies now in the literature. They have not yet come to anything definitive; but they seem on the verge of it, for the results of all of these studies point in the same directions. To put the trends simply: Those who stay in treatment improve; those who improve are better off to begin with than those who do not; and one can predict response to treatment by how well they are to begin with.

There have been many studies to predict response to treatment, such as that of Cartwright (15). Thirteen patients were given the Rorschach before treatment. Scoring was done according to the Rorschach Prognostic Rating Scale. The total weighted score predicted success significantly according to the counselor's estimates of improvement at the end of treatment. The author found that M, color, and form level could be combined to increase predictive efficiency. She generalized that the necessary qualities for improvement are "some combination of concern with real, human relations, the ability to empathize with others, well-controlled emotional responsiveness, and adequate reality contact." It is surprising to this reviewer as it has been to several other reviewers, that a simple prognostic rating scale can give significant results, both in this study and in previous ones. However, only 13 patients were used. Were it not for the results of previous re-

search on this same scale, it would be very hard to take these results seriously. Snyder's discussion at the end of the report raises the question whether the prerequisites for successful outcome in client-centered treatment are different from those for other therapies. There must be something in common for the different therapies, but this reviewer believes there must also be important differences between prerequisites for different treatments and even for short versus long treatments, and treatments of severely versus mildly ill patients.

In the *Counseling Center Discussion Papers* which are still mainly for private consumption, Cartwright shows successful prediction with the Thematic Apperception Test (TAT) on a somewhat similar basis. The Monroe-Hill Inventory (77) has been reported in another study as showing successful prediction of acceptability for psychotherapy in narcotic addicts.

A study of the correlates of staying in treatment is reported by Sullivan, Miller & Smelser (106). Groups of patients who stayed in treatment were compared to those who did not. There were no differences on the Minnesota Multiphasic Personality Inventory (MMPI) but those who stayed had a higher educational and occupational level than those who left. A study by Taulbee (109), using Rorschach scoring categories and MMPI, concludes that those who stayed in treatment were "less defensive and more persistent, anxious, sensitive and dependent. . ." They were also described as having more feelings of "inadequacy, inferiority and depression and better potential for self-appraisal, emotional responsiveness and more of an introspective attitude."

Another report of the psychotherapy research projects at Henry Phipps Clinic in Baltimore is provided by Gliedman *et al.* (35). Ninety-one psychiatric outpatients were included in the study, which has as its aim the finding of factors in the patient's expressed motives for treatment determining whether or not he will stay in it. Of the 91 patients, 28 left before the fourth treatment session. Motives were classified into two groups: traditionally "good" motives (e.g., relief of distress, improvement in interpersonal relations) and "poor" motives (e.g., primarily situational or environmental reasons). Such expressed motives were found not to be much related either to staying in treatment or to improvement. If anyone thinks that the patient's initial expressed motives for treatment alone are to be taken seriously in predicting staying or improving in treatment, he should consider the results of this study. There is also some evidence in the project's reports that the particular psychiatrist makes a difference in the patient's staying in therapy.

The same factors that seem to be related to staying in treatment are also thought by Gliedman *et al.* (35) to be related to improving. Sullivan *et al.* (106) report that patients rated improved were more likely to be those with higher occupational achievement and less psychopathology. Tolman & Meyer (114) found that improved and unimproved cases differed. The improved tended to stay in treatment longer, to complete treatment, to become

employed during treatment, and to be diagnosed psychoneurotic. Taulbee (109) also suggests that the same personality variables reported for staying are associated with improvement in treatment.

Tolman & Meyer made a study (114) of "who returns for more treatment," finding that those patients who returned were significantly less often those who were married than was true in an average group of patients. Those who returned were also less likely to be employed at the time they applied for treatment, and, by the termination of treatment, they held jobs as often as was true in the average group of patients.

The research group at the University of Chicago Counseling Center continues to make large and important contributions despite conceptual limitations. These limits derive mainly from the client-centered theory of therapy, which lacks a sufficiently complex account of the nature of motivation, neurotic conflict, and psychic structure. Cartwright (16) presents a neat example of methodological simplicity and clear-cut results in a study of the changes produced by psychotherapy in the self-concept. Ten patients were given the Butler and Haigh Cue Sort before and after treatment. The patient had to make three sortings at each time. His instruction was, "Describe yourself as you are in relation to each of three people who are very important to you such as father, mother, child, friend, your boss." As expected, the patients showed higher self-consistency in their self-descriptions in relation to other people after therapy than before. The more successfully treated patients increased in self-consistency more than the failure cases. Almost all the predicted relationships held exactly as expected in this small group of 10 patients.

One's view of relationships with people obviously ought to change after psychotherapy. TAT stories before and after treatment ought to reflect sensitively such changes. Another study from the Counseling Center of the University of Chicago (14) provides interesting information on these changes. For example, a pattern of relationships entitled "comfortable adjustment" was found to increase more for those patients who had longer therapy than for those with shorter treatment.

The price of methodological lack of sophistication is worth exemplifying by an example of an unreported study of a committee established by the American Psychoanalytic Association in 1952. The committee was charged with the task of surveying the important data of psychoanalytic practice. The method of choice, however, was a questionnaire with such items as age, sex, diagnosis, and degree of improvement. It was filled out before and after treatment by 800 participants, sending in 10,000 initial and 3000 final questionnaires. After this labor, it was recognized that there was not much contribution to knowledge possible from these data and in fact a lot of misunderstandings could be created. In this reviewer's opinion those undertaking the study had expected much too much from a single, superficial survey of opinion. Knowing the limitations of such methods would have

meant not investing in them in the first place, or going ahead with the full knowledge of their limitations, and so without disappointment. Perhaps, therefore, some of the results could have been presented emphasizing their "survey-of-opinion nature," or specific stratified samples of the data could have been studied more intensively, the data from the survey being used merely to establish characteristics of smaller samples selected for further study.

It has long been suspected that conflicting results from research studies carried out in different types of installations may have been caused by different populations. Haddock & Mensh (40) looked into the characteristics of patients attending three types of clinics: psychiatric services in university student health centers, other medical services in the centers, and a contract Veterans Administration mental hygiene clinic. The veteran sample was quite different from the other two. An interesting fact emerges that among the 1000 clinic patients in the three different settings, about two-thirds were in treatment for fewer than five hours.

People are always more alike than otherwise. Rubinstein (91) asks whether the factor patterns of normal and maladjusted individuals would turn out to be the same. A group of therapists and a group of psychiatric patients were rated on his "Multidimensional Scale for Rating Psychiatric Patients." He found that the factor analysis of the peer ratings of the psychotherapists produced similar patterns, although there were differences of degree between the psychotherapists and the patients. The factors emerging from the self-ratings were not as similar to the factors from the patient sample. The author concluded that the similarity between the factors emerging from the two groups lent significance to their stability and generality.

The usual diagnostic categories have been long recognized to be inadequate for judging the outcome of psychotherapy. Rubinstein & Lorr (92) attempt to develop personality types objectively by means of obverse factor analysis (that is, factoring to obtain types of people rather than traits). Ratings were obtained on 59 graphic rating scales in a group of nonpsychotic patients in outpatient psychotherapy. Nine patient types were isolated and described. An important finding emerged. The standard psychiatric diagnoses were not significantly related to these patient types. This attempt makes us less dependent upon standard diagnostic labels and is a method for getting an accurate assessment of the patient before and after treatment.

A sizable body of research utilizing the Interaction Chronoscope deals with the time pattern of speech and silence of patient and therapist. This verbal behavior of the participants was found to be highly self-consistent over a period of time (95) and the reliability of the observer was also great (83). Efforts are now under way to find personality correlates of these patterns.

Studies of the patient's life situation, particularly sociocultural influences.—There are no published studies taking up "situational variables"

in the sense in which the psychotherapy research project at The Menninger Foundation classifies this topic. There are no studies in which the main focus falls upon variables in the patient's situation, such as his family, his children, and his community as they interact with his ongoing psychotherapy.

A number of studies, however, are available which show that various social circumstances are relevant to the type of treatment the patient gets and whether he stays in it and makes progress in it. Future research on these problems will have to take account of a major investigation by Hollingshead & Redlich (43). It is a sophisticated sociopsychiatric survey of patients under psychiatric care in the New Haven community. The authors discerned five social classes on the basis of the combined criteria: area of residence, occupation, and education. Social class is definitely related to the number of psychiatric patients, types of disorders, and kinds of treatments obtained. As examples of the many interesting findings, the lowest class contributes proportionately more patients, and sizable status differences between the psychiatrist and his patient may play an important role in treatment failures.

That the treatment a patient gets is determined by much more than the type of illness he has is also claimed by Kahn, Pollack & Fink (47). They report their experience in a mental hospital which has many types of treatment available. Education, age, place of birth, and score on the California F (fascism) Scale were found to be relevant to the type of treatment given. The older, foreign-born, poorly educated patients with higher F scores were more likely to be given electroshock treatment. Those patients who were more like the therapists in these characteristics were apt to get psychotherapy.

Studies of physiological changes accompanying psychotherapy.—The reader who is especially interested in this type of study must be referred to the work of Lacey who has done a service in surveying and evaluating the research in this area for the Conference on Research in Psychotherapy (57).

Two studies, both emerging from Yale University, were reported this year. An interesting report by Dittes (20) showed that the patient's galvanic skin response (GSR) varies in relation to the therapist's permissiveness. Analysis of 42 interview hours of psychotherapy revealed that the patient's frequency of GSR increased as the therapist became less permissive. Dittes thinks of the GSR as a measure of anxiety or "mobilization" against any sign of punishment by the therapist. Auld, Dreyer & Dollard (5) describe assets of the apparatus they have developed for the measurement of electrical skin resistance during interviews.

Psychotherapy research methodology.—It has yet to be demonstrated that control groups in psychotherapy research have a more than very limited usefulness. The fact that one patient decides to go ahead and get treatment and another does not in itself makes for overwhelming incomparability. Imber *et al.* (46), of the Phipps Clinic psychotherapy research group, de-

scribe an improvement on the typical control group design. If the amount of time spent with the therapist is considered to be a major variable in producing change in patients, this can be varied systematically. Fifty-four psychiatric patients were randomly assigned to three psychotherapists who carried on three different kinds of treatment: group therapy, individual therapy at an hour a week, and individual therapy at one-half an hour every two weeks. The patients with the least time with the therapists showed the least change. In general this seems like a useful method for psychotherapy research. However, varying the time in this design does not solve all the problems of control. The "minimal" treatment could have been devalued by many of the patients and also by the therapists. Especially in an institution where some patients receive more intensive treatment than others, those patients seen less frequently feel rejected. A major question that still needs answering is: Was this devaluation responsible for the lack of effectiveness of the treatment?

THEORETICAL CONTRIBUTIONS

The theory of psychotherapy.—Among the experimental approaches, it is mainly the large-scale research programs that are able to make the best and most lasting contributions. This is not true of theoretical contributions or more purely clinical studies. It is interesting to note that despite the greater *rapprochement* in recent years between theoretical and experimental academic research, it is usually different people who do the two types of work. In general, the psychologists write experimental papers and the psychotherapists, theoretical or clinical ones. However, a countertrend is gaining ground as psychotherapists of various backgrounds and clinical and experimental psychologists participate in research projects and some academic psychologists extend their interests to the clinical theoretical side.

A fairly useful survey of recent developments in the theory of psychotherapy is provided by Thorne (113). He covers psychoanalytically oriented psychotherapies, client-centered counseling, eclectic counseling, disciplinary counseling, and other theories. Masserman (70) provides us with a set of broad generalizations, surveying contributions to the field of psychotherapy regardless of their recency. He classifies these in terms of historical, controlled experimental, and the bedside or clinical. Lewis (62) gives an historical account of the development of psychotherapy. Whitaker (121) edits a book in which we are offered the verbatim discussion of seven experts (including John Rosen, Thomas Malone, and Gregory Bateson) on the theory and treatment by psychotherapy of schizophrenic patients. Most of the contributions are aimed at identifying what the effective agents in psychotherapy are. A round table by Van Bark *et al.* (116) is devoted to discerning these.

Mendelsohn, Yates & Peterson (72) believe that the most important aspect of psychotherapy is the affect experienced in the interaction between

the patient and the therapist. Also, an atmosphere in which the patient is able to assume responsibility and make decisions is important. For Collier (18) the aim of psychotherapy is the "progressive re-establishment in the individual of a more adequate degree of self-regulation." The "pathologies of self-regulation" have been described by him in terms of the concepts of stress and defense. The author attempts to develop a regulatory theory of consciousness which will serve to integrate the phenomena of psychotherapy.

According to Raush & Bordin (86), warmth is a basic attitude on the part of the therapist. The authors examine the concept and its operation in psychotherapy. Three components of warmth are distinguished: commitment of the therapist, effort to understand, and spontaneity. The authors examine the similarity between these aspects of the therapist's role in relation to a patient and a parent's relation to a child.

It has generally been thought that old age is a contraindication to psychotherapy. Gruen (39) proposes that it often is an additional motive for psychotherapy. It should be assessed as a separate item in judging the motivation of a person for psychotherapy.

Some conceptual distinctions are attempted by Wyatt (127) that may help in understanding what is therapeutic in psychotherapy. A distinction is made between therapeutic effort (these are the explicit techniques of a therapist, such as questioning and interpreting) and the therapeutic situation, by which the author means the general circumstances and the interaction of the therapist and patient. Because of the special nature of the therapeutic situation, the patient can become aware that his responses are to internal pressures, not to external behavior, such as the actions of the therapist.

The theory of psychoanalysis.—Among the general contributions, several have been historical, as well as theoretical, reviews. Alexander (2) summarizes trends in our application of psychoanalytic views to psychotherapy. The development of psychoanalytic theory and technique is historically treated by Thompson & Mullahy (111). The authors acknowledge indebtedness to the views of Horney, Fromm, Sullivan, and Ferenczi, and emphasize the importance of their stress on the current situation and the contributions of sociology and anthropology.

The third of a three volume book on psychotherapy by Symonds (107) has just appeared. Volume I was devoted to the goals of and indications for psychotherapy and it discussed neurosis in everyday life. Volume II gave as its main thesis that the essence of psychotherapy consists of reactions of the patient. Volume III, the present volume, deals with the therapist's role in psychotherapy. Ten of the 19 chapters deal with problems of interpretation, a technique which is looked upon as the therapist's main tool. Interpretation is defined by Symonds very broadly. Other chapters are devoted to active procedures, such as reassurance, suggestion, the use of commands, persua-

sion, and advice. The book is mainly psychoanalytically oriented and combines the work of a number of analytic schools. It is both a primer of psychotherapy method and theory as well as a very comprehensive, well-written, integrated survey of the literature. Symonds is eclectic; he feels that there is something useful in many of the schools of psychotherapy. His book provides a useful ordering of our present information about psychotherapy.

A stimulating paper on the theory of psychoanalytic technique is contributed by Szasz (108). He re-examines some of the principal concepts upon which the theory of psychoanalytic treatment is based. The aims of analysis and the rules concerning technique are reviewed in order to comprehend better the underlying theory of treatment. A careful analysis of the place of interpretation in the theory of psychoanalysis is provided by Loewenstein (69). Walker (119) reviews five theories of how psychoanalysis works: the cathartic theory, the self-knowledge theory, the habit-relearning theory, the process theory, and the object relations theory.

Considerable attention has always been given to the analysis of transference because historically this has been the cornerstone of psychoanalytic technique. The following five papers on transference and countertransference are a good presentation of current analytic thinking on the subject. Nacht (80) affirms that the major aim of technique has been to keep the transference neurosis within bounds. For this purpose the attitude of the therapist must be "like a smooth surface offering no hold to an excessive transference." However, he recommends that at times when the transference neurosis becomes dangerously extreme, the analyst must deviate from this neutrality. Nacht believes that keeping to such neutrality under all circumstances may unduly prolong analysis.

It has been widely known for many years, especially since the work of Wilhelm Reich and the more recent work of Hellmuth Kaiser, that the patient's motor behavior can be a reflection of aspects of the transference. Krapf (55) describes how recognizing these motor reflections of the transference can be very useful in surmounting some kinds of resistance.

When patients, particularly schizophrenic ones, are treated as part of a "therapeutic community," Savage (96) points out, the diffusion of transference becomes a problem. He suggests various methods for handling this, such as conferences between the analyst and nurse, group supervision or conferences about the patient, and the passing on by the staff to the analyst of transference indications in the patient.

Since Freud introduced the term countertransference in 1910, there has been increasing interest in it, especially in recent years. Definitions however have varied widely from considering it to be the sum total of the analyst's behavior to the patient, to a more limited definition in terms of the analyst's own unconscious needs and conflicts as they influence the understanding or technique. Racker (85) considers it very useful for the analyst

to recognize that his reactions are responses to the patient. He tries to delineate the extent to which one can use one's emotions in response to the patient to further the treatment. His article is often couched in vague Kleinian language that makes it difficult to follow, but the reader should not be discouraged, for the ideas in it repay the effort.

A somewhat similar theme is expressed by Little (67). She traces the development of analysts' attitudes toward countertransference, which used to be thought of as something undesirable, to the present when the term is more and more accepted as something one can speak about in good company. Her plea is for determining the usefulness of the countertransference responses by being a little less cautious about using them in the treatment of the patient. It is interesting that nowadays therapists coming from very different backgrounds and theories are arriving at the same conclusions and beginning to act on them in their therapeutic techniques.

On theoretical and practical grounds it has been thought that psychoanalytic treatment could not be carried out properly unless the patient paid a fee. Absence of payment would have many meanings to the patient which would cripple the analysis. The transference neurosis would not be able to be dealt with properly because the patient would be unable to show his negative feelings. The results of a clinic operation in which no fees are charged is described by Lorand & Console (68), who report that in their experience such treatment is possible.

SPECIAL APPLICATIONS AND TECHNIQUES

New techniques of psychotherapy.—It is surprising how many techniques are offered each year as novel and yet none of them strikes one as very new. Berne (11) describes a therapeutic approach aimed at "structural analysis" to strengthen the boundaries between the various ego states and help the adult part of the ego to be an ally in the treatment. Percentages of cures and failures are presented. Foulkes & Parkin (31) describe a new system, combining individual and group psychotherapy for outpatient psychotherapy at the Maudsley Hospital in London. A recommendation by Burgum (13) is in line with the new trend to use the therapist's responses to the patient therapeutically. The author feels that allowing the patient to know the therapist's values may assist the patient. Several contributions take up ways of dealing with the families of patients in psychotherapy (1, 74, 122).

Psychotherapy combined with drugs.—Psychotherapy is frequently combined with physical agents such as electroshock treatment and drugs. Esecover, Jaffe, & Kahn (27) describe the way electroshock makes patients more able to participate in interpretive forms of psychotherapy. All four of the reports in which psychotherapy is combined with drugs are presented to show that with selected cases, drugs make psychotherapy more effective or even make it possible where it would not be otherwise. The drugs de-

scribed are thiobarbiturate and analeptic drugs (90), lysergic acid diethylamide (61), chlorpromazine (6), and antabuse (124).

Treatment of children and adolescents.—The contributions dealing with children and adolescents are roughly divided into two groups, those with important theoretical implications and those that are primarily concerned with technique and are either descriptions of treatment programs in various institutions or guides to treatment.

Levitt (63) presents a report of a method which is fortunately becoming rarer. His study is in the Eysenck style of improvement ratings for a treatment group compared with control groups of nontreated children. For what it is worth, the conclusion is that psychotherapy with neurotic children is ineffective. By contrast, the article to follow by Ekstein & Friedman (22) has a tremendous amount to teach us about psychotherapy with children and adolescents. This article is one of an entire issue of the *Journal of the American Psychoanalytic Association* devoted to problems of acting out during psychotherapy. The article is a fine example of the best kind of use of single cases from which to derive greater understanding of changes produced by psychotherapy. Excellent single-case presentations of this kind are becoming more rare in the literature of psychotherapy. The presentation describes the treatment of a 13-year-old boy who had been caught by the police following an act of petty thievery. The principal way in which the boy could talk to his therapist was through his acting out, play action, and eventually play acting. The authors provide systematic definitions of each of these three. Usually in the treatment of adults, acting out is considered a necessary obstacle to be removed by interpretation or suggestion. For this patient it was thought that the acting out, play action, and play acting, rather than only taking the place of remembering, "represented experimental recollection." The fact that the therapist accepted this type of communication made it possible for the patient to imitate and then to identify himself with the therapist. The aim of the authors was to relate questions of technique to the problems presented by "a psychic structure which was dominated by acting out."

There is another in a series of studies by Ekstein and his associates, whose work has done much to demonstrate how and why borderline psychotic and psychotic children are or can be brought to be amenable to psychotherapy. Ekstein & Wallerstein (23) show that progress depends upon developing a language to begin the relationship. The problem of the choice of interpretation to be given to the child is determined by knowledge of the ego fluctuations of these children. These ego states vary from ones in which the child can brook no delay to ones where he can use thinking maturely as trial action. Gondor (37) also demonstrates in psychotherapy of children the importance of understanding and working with the child's fantasies as a means of communicating with the child. Alpert (3) describes the techniques necessary in prelatency children, which include "giving the child a

controlled primary, need-oriented relationship to work through the developmental stages not mastered in its primary relationship with the mother."

Three reports are devoted to the way controls can be provided in the psychotherapy of adolescents. Wilkins (123) describes a schizophrenic adolescent girl in whose treatment control of aggressive behavior played a very important part. It was of "great value to the patient to find the therapist capable of supporting the healthy ego against the impulses that threatened to destroy the patient, therapist and treatment." Noshpitz (82) presents the rationale of showing "sympathetic concern with the details of the patient's antisocial activity" for his technique with adolescents with character disorders. The therapist shows he is quite able to understand these activities, to speak about them, and to contemplate doing them himself. Kaplan *et al.* (48) present their theory for controlling acting out in the psychotherapy of delinquents.

General presentations of psychotherapy with adolescents are presented by Balser (8) and Boenheim (12). Descriptions of treatment programs in various institutions are plentiful (28, 41, 49, 75, 89, 97, 101).

Group psychotherapy.—Topics covered under the first six studies of group psychotherapy to follow are quite like those covered in the research studies of individual psychotherapy. Ullmann (115) investigates how to choose patients for group psychotherapy and derives measures from six cards of the TAT which give a better prediction of hospital status six months later than rating of overt behavior. Nash *et al.* (81), as part of a project previously described in this review (35, 46), list some factors related to patients staying in group psychotherapy. The fact that chronic schizophrenic patients improve in group psychotherapy in their interpersonal functioning is shown by Semon & Goldstein (98). Two methods of group therapy were compared, "active-participant" and "active-interpretive." No differences could be found in the success of these. Three kinds of treatment were attempted with alcoholics by Ends & Page (26). The three types were analytic, client-centered and learning theory, the last showing little effectiveness compared with the other two. Decision making seemed to be facilitated by group psychotherapy, according to Kimber (51). Facilitation was also produced according to Bindelglas & Gosline (10) in group psychotherapy by reserpine and chlorpromazine.

General reviews of history, theory and practice of group psychotherapy have been done by Corsini (19), Foulkes & Anthony (30), Mullan (79), Dreikurs *et al.* (21), and Kubie (56). A variety of special problems dealing with the technique of group psychotherapy and the selection of patients are available (9, 34, 45, 52, 54, 60, 63, 64, 65, 66, 73, 74, 78, 110, 117).

Teaching and supervision of psychotherapy and psychoanalysis.—As the public demand for psychotherapy has increased, the professions which have it to offer have become more interested and expert in teaching and supervision methods. The method that is in most use with the most satisfaction

reported by the participants is that of individual supervision. In the Menninger School of Psychiatry, for example, residents reported that they considered individual supervision the main way of learning psychotherapy (44). Other methods, such as group supervision and teaching, were considered by them markedly inferior. An excellent book on the subject of problems of individual psychotherapy supervision by Ekstein & Wallerstein (24) will be published in a few months.

Rogers (87) stresses the importance of experiential as opposed to cognitive learning in developing skill in psychotherapy. He discusses the values of methods he has tried. These are listed roughly in order of their usefulness: listening to recordings, "pairing-off" students, sound motion pictures, appropriate methods of teaching, therapy conducted before a group, "participation" in group therapy and individual therapy, early experience in helping relationships, play-backs of the students' interviews, multiple therapist experience, and supervised experience with psychotherapy.

A variety of new methods have been tried. Fleming & Hamburg (29) report success with a method of presenting the raw data of an interview to a class. The students have a chance, under the leadership of the teacher, to try their hand at interpreting the interview, as well as suggesting responses they would make if they were the therapist. Another method is attempted by Gans (33) in which two therapists work together as co-therapists in the psychotherapy of a group of patients. Each therapist observes, is observed, and then participates. A program of psychotherapy supervision is described (118) and a case example is given of the supervision of the psychotherapy of a schizophrenic patient (99). A book by Balint (7) should be of considerable use for general practitioners. It contains lectures given for the training of physicians in psychotherapy in general practice. Much of the emphasis is upon how the doctor can use his own response to his patients and their illnesses therapeutically. Alexander (2) considers in his book problems of training in psychiatric institutes and medical schools.

REVIEWS

Before mentioning reviews specific to psychotherapy, it should be noted that there are reviews of the field of psychiatry and psychoanalysis that include psychotherapy. A monumental reference work indexing all psychoanalytic writings is being published (38). It used to be that if one wanted to find out what work had been done on a psychoanalytic topic, Fenichel was a fairly adequate source book. With the burgeoning of the field, this book has become insufficient. The new index comprises five volumes of references, covering the entire psychoanalytic literature through 1952. There are approximately 37,500 listings, alphabetically by author and title, with a cross index of subjects. It should be of great value to anyone who wants to find quickly what is written on any psychoanalytic topic. The *Annual Survey of Psychoanalysis* should also be useful (32). It offers con-

densations of all articles published. The coverage is fairly complete and the reviews are conscientiously done. Another volume, called *Progress in Neurology and Psychiatry* (199), also gives coverage in the fields of neurology and psychiatry annually.

A brief annual summary of progress in psychotherapy is given by Hoch & Lewis (42). Changing concepts of therapy in a mental hygiene clinic are described by Pumpian-Mindlin (84). The University of Kansas Department of Psychology produced a report (126) of an institute on research in clinical psychology, devoted to "research on the process and outcome of psychotherapy." This report includes papers and discussion by Sargent, Luborsky, Korner, Rogers, and Robbins.

It has now been decided to issue annually a volume entitled *Progress in Psychotherapy* (71). Volume II includes the proceedings of the 1956 section on psychotherapy of the American Psychiatric Association, as well as other papers. It contains some interesting material, written by competent people. However, a great deal of this has been printed before and was not very new then. The book begins with an introduction by Moreno; it has a large section of papers on analysis of anxiety and a section on schools and trends in psychotherapy, including developments abroad. There is a final, interesting chapter by Masserman which has also been separately published (70).

SUMMARY OF TRENDS

The body of research in psychotherapy today is, appropriately, loosely draped in the current fashion called "process research." At the Washington conference on psychotherapy, only two representatives of outcome studies participated: the project represented by Frank (35, 36) and The Psychotherapy Research Project of The Menninger Foundation (120). The latter actually bridges the two types and is an "outcome-process" study. The change in fashion has at least one good reason; the hundreds of outcome studies in recent years, with their percentages-of-improvement type results, have led to general disillusionment in such studies. There is more interest now in what kinds of changes occur in the course of psychotherapy, especially changes that can be discerned in the interactions of patients and therapists. For example, the work of Rogers (88) in creating scales for scoring interviews mainly attempts to capture dimensions of change in the patient in the course of treatment. Other important research tools in the form of reliable systems for coding the processes of treatment are being developed by Strupp (103) and Leary & Gill (59). The latter gives promise of being the first major reliable content scoring-system embodying psychoanalytic concepts. However, there still is much need for intensive studies of what changes are achieved through psychotherapy.

While the therapist has come under investigation more in recent years, our knowledge in this area is still rudimentary. Very likely there would be considerable agreement about necessary qualities of therapists only if the

propositions were sufficiently general. This would be true, for example, of Strupp's conclusion (105) that we need a therapist with human qualities, such as warmth, plus knowledge of therapeutic technique, before the patient can achieve maximum benefit. We are coming to the stage where a few patient variables have been found to be related to treatment outcome. Remaining in treatment, and certain favorable qualities such as concern with relationships, ability to empathize, and emotional responsiveness [as reported by Cartwright (15)] are related to improvement in treatment. Hollingshead & Redlich (43) have carried us a major step further in new knowledge of the "situational variable," social class. This reviewer advocates more studies in which the interaction between all four major types of variables can be adequately assessed.

A number of rigidly quantitative studies which focus upon counting of words and silences or use an apparatus like the interaction chronoscope appear to be less fruitful than those which stick to more essential aspects of psychotherapy. These aspects of treatment seem to have been chosen primarily because they are more readily quantifiable. They are reminiscent of the now classic story of the logic employed by the drunkard looking for his lost house key under a lamp post only because there was more light there.

Most research reports deal with short-term psychotherapy. It is undoubtedly easier to carry out a research study in psychotherapy when the treatment is short. However, it is important to know whether the same kinds of findings would emerge from long-term treatments as from the shorter type. In addition, too many of these short-term treatments are carried out by inexperienced therapists. It would be difficult to maintain that the product of such treatment would be the same as one carried out by an experienced therapist for hundreds of hours.

In recent years there have been noticeably fewer single case reports of psychotherapy. We do not refer here to the publication of verbatim recordings of treatments—these have increased in recent years—but to case examples in which the author tries to answer a question posed by a theoretical position. The marshalling of evidence in a case report such as Ekstein & Friedman (22) achieve is useful for developing a theory of treatment. It is as if experimenters have despaired of learning anything from single patients and have gone in for the model of research, implied by the custom in academic psychology circles, of taking groups of patients and describing characteristics that apply to the aggregate. The development and application of methods for the experimental study of single patients still seem desirable. The P-technique and other methods for studying intraindividual variability should prove very useful but are little used.

Obstacles in the way of good research in this field are hard to move. It is difficult to get a project started. Then it is hard to keep it going. Once it is done, it is hard to get anybody to consider that it is relevant to what he is doing. Some of these obstacles have been dealt with excellently in

an article by Mitchell & Mudd (76). The authors detail the problems of getting recordings of psychotherapy and facilitating the cooperation of patients and therapists. They report what is becoming by now a common experience, that patients are much more ready to participate in research projects than therapists.

Actually, some of the resistance to applying recent research findings in one's practice of psychotherapy is healthy resistance. Little that derives from experimental research literature in psychotherapy has been established with great certainty and bears on an issue that is vital in our practice. This is true even of the best designed of the experimental studies. It, therefore, might even be dangerous to apply research findings until they are confirmed and integrated into the body of our knowledge.

It is, however, slightly easier nowadays than it used to be for therapists with different backgrounds to exchange ideas, despite the fact that there are still definite and difficult barriers. As Klein (53) pointed out almost ten years ago, the barriers between the clinical or applied and experimental or academic investigations were even then becoming less clear-cut. After all, while the therapists have different backgrounds, they often start from dynamic conceptions that ultimately derive from Freud. Furthermore, psychiatrists and psychologists of today have had considerably more experience in psychotherapy than their predecessors of ten years ago.

The publishing of reports in the literature has very definite shortcomings as a method of communicating with one's co-workers. In the Washington Conference, even after prior reading of the papers to be presented, the actual presentation by the author and discussion with him made a noticeable difference in the participant's ability to take the work seriously, and even to become fully acquainted with it. Such use of the conference method can be one small way to facilitate exchange of ideas and appreciation of work done by investigators with different backgrounds and concepts.

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COUNSELING^{1,2}

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The definition of counseling discussed by Tyler (102, 103) is more oriented toward what counselors hope they do than toward what they have been observed to do. Tyler has attempted to distinguish counseling from psychotherapy by limiting the meaning of the former term to the process aimed at helping a person obtain a clear sense of personal identity. She says, "Counseling aims at acceptance of assets and limitations rather than at fundamental personality change." The limited range of activities included in this definition can be compared to a somewhat similar limitation included in the definition of psychotherapy by Colby (23): "The goal of psychotherapy is to relieve the patient of distressing neurotic symptoms or discordant personality characteristics which interfere with his satisfactory adaptation to a world of people and events. . . . A psychotherapist should not expect a great transformation equivalent to a psychological rebirth or a complete reorganization of the patient's personality." The definition of counseling and its distinction from other related activities still await empirical investigation.

Counseling is an art based on knowledge and skills derived from many sciences, including those psychological, but in and of itself it does not constitute an entitative science. Counselors engage in much scientific activity, and scientific research involves much counseling behavior, but most of the research of direct relevance for counselors properly can be included in better demarcated scientific specialties such as psychological measurement, personality dynamics and psychopathology, social psychology, learning, educational research, sociology, economics, and other social sciences. A few research studies, usually interdisciplinary, properly can be labeled "counseling research."

When this author (7) reviewed current research in counseling nine years ago, he concluded, "An area of research of importance to every counselor, but in which this reviewer could find no research publications, concerns the relationship between diagnosis and therapy. What diagnostic categories and techniques are most useful in selecting appropriate therapies?" Near the conclusion of this nine-year period, one group of authors (3) reported the first substantial study of therapy that took into account the effects of precounseling counselee attributes, differential counseling methods, and outcomes. Although the results of this study were rather meager, certainly the study itself should be considered a pioneer one. It makes one more

¹ This review covers the period from April, 1957, to April, 1958.

² The following abbreviations are used in this chapter: ACE (American Council on Education); MMPI (Minnesota Multiphasic Personality Inventory); SRA (Science Research Associates).

hopeful about the knowledge we may acquire during the next nine years.

Pennsylvania State University psychologists, Ashby *et al.*, studied ten counselors, each of whom worked with two counselees using a nondirective approach and two additional counselees using an interpretive approach. Relationships were determined between counseling behavior and precounseling measures of deference, autonomy, succorance, dominance, and aggression (all obtained from the Edwards Personal Preference Record), tolerance-intolerance on cognitive ambiguity tests, and the number of problems checked and number of words used on the Mooney Problem Check List. Correlations were determined between client pretherapy personality characteristics and client reactions during therapy and between client's reactions in therapy and therapist personality characteristics. The analysis indicated that counseling behavior properly is conceived of as an interaction between client personality dynamics, kind of counseling used, and personal characteristics of counselors. Some evidence suggested that the interpretive type of therapy had a different effect upon counselees than did the nondirective therapy. Perhaps most important was the conclusion that counseling research must take into account the more subtle interactions, both clinically and statistically, that constitute counseling.

A somewhat similar study (but one that made no attempt to study the differential effects of various counseling methods) was partially reported (9, 13). This again was a complex, carefully controlled and rigorously designed statistical study of the effects of counseling performed in a typical university counseling setting, taking into account counselee expectations and precounseling characteristics. Hoyt, Magoon, Volsky, and Norman at the Student Counseling Bureau of the University of Minnesota devised and carefully standardized objective measures of anxiety level, defensiveness, and personal problem solving. Using eight counselors and 100 counselees, 20 of whom provided a noncounseled control group, they studied pre- and postcounseling changes on the three types of instruments. A superficial statistical analysis revealed significant differences, but when more appropriately complex statistical controls were applied, no evidence of change related to counseling was found on the tests. The results of this study, along with the results of earlier studies done by Magoon and others, suggested that relatively brief-term counseling as done in most university counseling centers cannot be evaluated on the basis of changes in personality characteristics; rather, evaluation must be made in terms of more specific behavior changes. The average university counselor who sees his counselees about three times (the average number of interviews each client has in most counseling centers) perhaps should hope only to help his students make certain course selections, take first tentative steps toward changes in social behavior, or learn how to handle difficult but fairly specific family situations. As these relatively elemental problems are overcome by counselees, we hope that their progress toward personal fulfillments and increased insight will be facilitated, but expecting changes in larger goal-related behaviors may be naively optimistic, or even presumptuous.

COUNSELING RESEARCH IN PSYCHOLOGICAL MEASUREMENT

The frequency with which counselors use various tests was suggested in a paper by Silvania (89), who surveyed tests used in 167 counseling centers approved by the American Personnel and Guidance Association. His check list of 155 tests was returned by 141 centers. Of all intelligence tests, the Wechsler-Bellevue and the American Council on Education Psychological Examination were most frequently used. The most frequently used tests of various types were the following: achievement, The Stanford Achievement and the Iowa Placement and Achievement Tests; vocational aptitude, the Minnesota Paper Form Board and the Bennett Mechanical Comprehension Test; vocational interest, the Kuder Preference Record and the Strong Vocational Interest Blank; and personality, the Minnesota Multiphasic Personality Inventory and the Rorschach.

A factor analysis of the Wechsler Adult Intelligence Scale using the standardization data for four age groups, 18 to 19, 25 to 34, 45 to 54, and 60 to over 75, was done by Cohen (22), who found the same three correlated factors reported in previous studies, with a strong general factor accounting for about one-half the total variance. In general, the factor structure of this test appeared to be fairly constant from the late teens through old age. Some indication was found that the factor structure of the test for the 60 through 75 years group underwent some change, with the memory factor being more evident for the older group. The author interpreted his finding as failing to support Garrett's "differentiation hypothesis." For normal adults between 18 and 54 years of age, this test tends to serve a constant purpose. A study (96) of scores on the Primary Mental Abilities Test earned by 50 college graduates from 70 to 88 years old revealed evidence of differential decline in abilities with the earliest and largest losses on tests of memory, speed, reasoning, and spatial abilities. Verbal meaning and numerical ability scores were at least as high as those for average 17-year-olds in a majority of subjects over 80. Nadler (71) studied the Wechsler Adult Intelligence Scale with 53 severely handicapped persons employed in a sheltered workshop. He found ratings of job performance correlated .51 with verbal IQ and .54 with performance IQ. Considering that the reliability of the ratings was .70, the correlations were reasonably high. The mean IQ's of his group were: verbal IQ, 93, performance IQ, 88. Thus, this test seems to have some use in predicting the performance expected of persons in sheltered workshops.

A relevant study done a few years ago but not included in these Annual Reviews bears closely upon one problem facing counselors. Plaut (79), working with the National Scholarship Service and Fund for Negro Students, reported a program which involved the testing of 3178 students from 78 Negro high schools in 45 of the largest Southern cities. These students, selected by their principals and counselors as constituting the top ten per cent of their senior classes, were given the Scholastic Aptitude Test of the College Entrance Examination Board. Of these students, 1732 qualified for college work, 578 applied for college admission to interracial col-

leges, and 523 of these were accepted by a college. Most of these later were enrolled in 138 nonsegregated colleges throughout the country. Of this total group of students in the top ten per cent, only about 55 per cent had test scores that indicated a favorable prognosis for college success, the test scores ranging from below the first percentile to the 98th percentile, using national norms. Plaut reported that the overwhelming majority of all of the students who went to college made a successful academic adjustment in these nonsegregated colleges, in spite of the fact that they received their high school training in segregated high schools. The higher the socioeconomic status of these students as measured by their father's occupation and their father's education, the higher were the test scores. Students of lower socioeconomic status, who generally had low test scores and who ordinarily would be expected to receive lower grades in college, did better than was predicted. The results of this program review suggested that the prediction of academic success on the basis of test scores is even more difficult for persons coming from culturally deprived homes than it is for other students.

A comprehensive description and review of multifactor tests (111) now used by counselors was published as a series and later as a separate publication by the American Personnel and Guidance Association. The tests reviewed included the Differential Aptitude Tests, the General Aptitude Test Battery, the Guilford-Zimmerman Aptitude Survey, Unifactor Tests, Primary Mental Ability Tests, Factored Aptitude Series, Multiple Aptitude Tests, and Flanagan Aptitude Classification Tests. The test descriptions by the test authors and the comments by Super provide present and prospective test users information essential in understanding the purposes of the test constructors and the effectiveness with which the tests achieve these purposes.

Counselors who use the Differential Aptitude Tests will be interested in a study reported by Mendicino (66) who compared scores on the mechanical reasoning test and the space relations test of an experimental group of 150 tenth grade boys who took courses in vocational machine shop and mechanical drawing to scores of a control group who did not take the courses. The experimental and control group were comparable on the basis of age, race, and physical condition, and the two groups were tested during the first two weeks of the school year and at the completion of the school year. Means for the two groups on the initial and final tests were almost identical. The author concluded that the experience of one school year in vocational machine shop in the tenth grade did not increase intrinsic mechanical reasoning or space perception.

Guilford and his students (11) continued their research on imagination and related creative abilities, studying the planning abilities of 364 United States Air Force air crew trainees. A factor analysis of a battery of 52 tests revealed 17 factors, and orthogonal rotation identified 14, four of which were new. The results indicated that planning ability consists of a large number of primary abilities, some of which may be quite unique to

planning tests. Guilford's long and admirable series of studies as yet has not reached the point where it is lending itself to a counselor's use.

Of more immediate relevance for the counselor was the study by Moltenkopf (69) who selected from 100 items in a basic mechanical ability test 52 items which were easier for women. He found that the validity coefficient of the test specially constructed for women was .47, as compared with a validity coefficient of .39 for the original test. Although this study was directly concerned with problems of testing enlisted women in the Navy, it deals with a question which concerns the appropriateness of using for women mechanical ability tests which are standardized on men.

INTERESTS

Muller (70), one of our most distinguished zoologists, presented a paper in which he proposed that the survival of the race has been influenced by genetic natural selection as it applies to values, likes and dislikes, and through inference, interests. He proposed that certain types of behavior are conducive to survival. One group of these behaviors is related to intelligence, another to cooperative behavior. "We must admit that it is much too early for detailed formulations of the place to be accorded to the diverse major and minor values that flow out of his numerous inherent affective tendencies and out of the possibilities of interconnecting them, modifying their modes of instigation and expression, and enhancing some and subordinating others. For this purpose we have as yet far too little knowledge of just what these tendencies are, and of their genetic and acquired variations, in neurological, psychological, and sociological terms." The implications for interests are obvious.

Roe (82) continued her interesting speculations concerning determinants of vocational choice. She emphasized the importance of early satisfactions and frustrations in the development of interests and the primarily unconscious needs that determine the nature of these interests. She presented a schema which includes the child's pattern of early experiences with parents, the relation of parental attitudes and need satisfaction, and the style of parental handling of the child. This schema leads to the prediction of the broad and general vocational orientation that will develop in the individual.

Using a somewhat different approach, Strong (95) administered the Strong Vocational Interest Blank to a group of male Stanford students and several years later tested the sons of these ex-students. Pearsonian correlation coefficients between interest profiles, each containing 36 scores, for the 182 pairs ranged from -.67 to .92, with an average of .41. The mean correlation between profiles of older and younger subjects paired by chance was .24. The difference of the correlation between .24 and .41 is that which can be attributed to the parent-son relationship. Strong concluded that about one-half of the 182 students were similar in interests to their fathers, 5 per cent were dissimilar, and about 41 per cent had interests approximating a chance relationship to those of their fathers. Taking into account the correlations between chance pairs, about one-fourth of the sons had interests

similar to their fathers' interests; the remainder resembled their fathers no more than chance would allow. The correlation between the occupation of the fathers and the occupational plans of the sons averaged .48.

Additional information concerning the nature of interests was provided by Minor & Neel (68) who compared the achievement motive (McClelland) to the primary occupational preference of 50 male veterans of the Korean War. A statistically significant relationship was found; persons with highest achievement motives preferred occupations with higher status. Interestingly enough, subjects with moderate and very low achievement needs tended to have more realistic vocational choices than those with very high achievement needs.

Three studies provided some information directly about the validity of the Strong Blank. Hoyt & Kennedy (45) compared the scores on the women's Strong Blank and on the Edwards Personal Preference Schedule for a group of 30 career-oriented college freshman women and 71 homemaking-oriented women. The homemaking-oriented girls averaged significantly higher than the career-oriented girls on the Strong scales for buyer, housewife, elementary teacher, office worker, stenographer-secretary, business education teacher, home economics teacher, and dietician, and significantly lower on scales for artist, author, librarian, psychologist, physical education teacher, and physician. On the Edwards scale, the homemaking group scored higher on heterosexuality and succorance and lower on achievement, intraception, and endurance. The authors proposed that the differences in interest scores reflect differences in basic needs or motivations.

Hewer (43), taking advantage of a suggestion made by Segel in 1934, computed a statistically significant correlation for premedical students between scores on the physicians scale of the Strong Blank and differences between honor point ratios in natural science and social science courses. Although a group of premedical students with high scores on the physicians scale tended to get no better grades in their premedical work than a similar group matched on the basis of ability but without high scores on this scale, the correlations between differences in grades and interests suggested the need to consider differential achievement when one is using differential interests for prediction purposes.

Dunnette (29) developed four new scoring scales for the Strong Blank by attaching weights not to the item responses but rather to the occupational scale scores. The four functional scales, skilled research scientists, applied research and developmental engineers, process production engineers, and sales and technical service engineers, correctly classified 62 per cent of the subjects. No real differences were found between the standardization and the cross-validity samples, and the tetrachoric correlation was about .70. This attempt to find subcategories of engineering interest based upon functional difference appeared to be more promising than earlier attempts which divided engineers into groups on the basis of specialties, i.e., civil engineer-

ing, electrical engineering, mechanical engineering, and which used as a point of reference men-in-general.

Three relevant papers appeared during the year on the methodology of interest measurement. Kuder (60) compared various methods of constructing an occupational interest scale using Kuder Preference Blanks (occupational) completed by 1250 members of the American Psychological Association and a cross-validation group of 317 psychologists and by 400 and 100 pharmaceutical salesmen.

Eleven different scales were developed for each of the two occupations with variations in method of scoring, pattern scoring, answer position scoring, weighted scoring, and unit scoring. Point-biserial correlations of the 11 psychologist scales varied from .73 to .78 and for the pharmaceutical salesmen scales from .63 to .66. Kuder concludes, "The most remarkable feature of the results is the comparatively small range covered by the validities of the keys for each occupation . . . it appears that the relative effectiveness of the different possible ways of building a key is a complex function of a large number of variables which probably include the number of cases, the composition of the inventory, the content and type of item, the range of item validities, the homogeneity of the groups, and the extent to which the items can be considered to be uniformly distributed in the domain represented." We still know relatively little about the best possible way to construct an interest scale.

Cooper (25) studied the relationship between the group scores on the Strong Blank and primary interest patterns as determined by occupational scores. His results suggested that a low score on the group scale was indicative that one would not find a primary pattern in the related group of occupations, but that a high score on the group scale was likely to mean that a primary pattern would be found. Differences were found among the various groups themselves. The results of his study provide further discouragement for the use of the group scales at the present time.

Two studies were concerned with the problem of stability of scores on the Strong Blank. Hoyt & Smith (46), extending the earlier work of Stordahl, proposed that the extent to which a person's interest profile could be called integrated was related to the degree to which his scores on all of the other occupational scales resembled the scores obtained by the criterion group for that scale on which his score was highest. Three measures of integration were derived: positive direction, concerned with high scores; negative direction, concerned with low scores; and depth of integration, or congruency of high and low scores. These indices were related to measures of change first over a two-year period and then over a four-year period. The results supported the hypothesis that the greater the integration of the individual interest profile, the greater the stability. In all cases, however, the coefficients of correlation were low, ranging from .27 to .37.

In a related study, King (55) retested with the Strong Blank 242 college freshmen who had been tested a year earlier. He compared two new objec-

tive indices of profile stability, one based on letter grade changes and the other based on letter grade and pattern changes. These two new methods were compared to Power's D-score method, ranked correlation coefficients, and counselors' ratings of the extent of interest change. The stability measures were all significantly intercorrelated. The four statistical methods correlated with the counselors' ratings from .55 to .68. The author concluded that the new methods might be preferable to the old methods on the basis of the extent of agreement with counselor ratings. As yet, no satisfactory method is available for identifying in advance those interest profiles most likely to change upon retesting.

A study (39) of the fakability of the Strong Blank was concerned with the specific question as to whether engineering students could first take the test to reveal their own interests, and then later take the test so that they could discard their own interests and obtain high scores on occupational group 5. On the first test, 46 of the 60 persons obtained scores of above 37 on occupational group 2, whereas in the second test, all the scores were below this point. On the first test, 35 of the persons obtained scores below 34 on group 5. On the second testing, all of the subjects obtained scores above this point, thus again demonstrating that interest scores can be faked.

Continuing attention was given to occupational interests as exhibited through vocational choice. Strong (95) presented information about the occupational choices made by his college students in 1935 as compared with the occupations in which they were engaged in 1949. The coefficient of correlation was .84. This figure tends to agree with the previous estimate presented by Strong, based on more comprehensive data.

Stephenson (93) pointed out, as others have done, the need to distinguish between vocational plans and vocational aspirations. He gave questionnaires to 1000 ninth grade students and compared their occupational plans and aspirations with their educational plans and with the distribution of occupations in the national labor force. His paper raises many relevant questions concerning the theory and practice of vocational choice. Somewhat similar questions are discussed in a paper by Patterson (78) who studied the problems of vocational choice among emotionally disturbed persons. He tends to reject the value of psychoanalytic theory in gaining an understanding of this process.

Other authors have given attention to the problem of interests and of emotional stability. Klugman (56) analyzed the scores made by 100 male psychotics on the Kuder Preference Record and found significant differences between the psychotic group and Kuder's group of supposedly normal persons only on the mechanical score. This should serve as a warning to persons tempted to overgeneralize from previous studies concerning the differences in Kuder interest patterns between normal and emotionally disturbed persons.

PERSONALITY MEASUREMENT AND COUNSELING

Among the many reports on personality measurement appearing recently, some are particularly relevant for the counselor. Several of these pertain

specifically to the validity of instruments in current use. Drake & Oetting (28) continued an extended series on the Minnesota Multiphasic Personality Inventory as used with university students. They reported the effect of the Mf scale as a suppressor variable upon academic achievement of students who had high scores on Ma and Sc and low scores on Si. In the total freshman group, 41 per cent had below C averages. In the group with high Ma and Sc and low Si without high Mf, 67 per cent had below C averages; in the group with high Ma and Sc and low Si with high Mf, 18 per cent had below C averages. The Mf score by itself, or in conjunction with the ACE score in a multiple correlation, did not significantly add to the prediction of academic grades. Results of this study strongly suggest that high scores on Ma and Sc with low score on Si can be used to assist in academic predictions provided one takes into account the level of the Mf score. This study done on University of Wisconsin male students needs replication in other universities.

MMPI scores, along with other variables, were compared for university students apprehended for theft and a control group matched on sex and class by Jackson & Clark (48). The two groups did not differ on ACE scores, but the control group obtained significantly higher grades and higher scores on D, Mf, and L, with lower scores on Pd and Sc. The two means on Pd were 60 and 54.

Swan (98) studied the relationship between MMPI scores and patterns and marital adjustment as measured by a 23-item "marital adjustment scale" he developed. The more happily married couples scored lower on Pd, Pt, and Ma and on an internalization ratio and higher on the Re scale. The mean scores on the latter two indices rose for those spouses who were more dissatisfied with their marriage than were their partners. The subjects had been given the MMPI and the Marital Adjustment Scale two years after their marriage, on the average. The results of the study support the evidence of relationship between scores on the MMPI and marital adjustment.

Calvin & Hanley (17) administered the MMPI to 300 undergraduate students and from this group selected 9 who appeared to be faking good on the basis of Cofer, Chance, and Judson's key and 4 who appeared to be faking bad on the basis of Gough's F-K index. Each of these 13 subjects was matched with a control subject having a similar raw score profile but with no evidence of faking, and all subjects were again given the MMPI while being tested with a polygraph. Polygraph records of the faking groups were different from the records of the control groups, and the evidence suggested the scales used on the MMPI for detection of faking were of practical value.

Stone & West (94) studied 1159 men and women who had been tested with the MMPI during the first day of college. After five months, 127 students with high scores on Ma, Pa, and K were retested along with 14 randomly chosen "normals." Of the 127 students in the experimental group, only 38 per cent had "abnormal" profiles on the retest. No significant changes in the profiles were found for the control students. The authors concluded that the scores obtained from university students during the early

part of the school year were not representative of the scores that would be obtained later.

Jensen (50) attempted to relate MMPI scores and academic achievement, as indicated by agreement or disagreement between predicted grade point average and achieved grade point average for 458 Brigham Young University freshmen. On 6 of the 11 scales, the low ability nonachieving students obtained higher mean scores than the high ability achieving students. On three scales, the nonachieving low ability students were higher than the achieving low ability students. The nonachievers of low ability had higher scores on five scales than did the nonachievers of high ability. In general, nonachievers of low ability tended to obtain higher scores than all of the other groups, with a marked exception for scores on the *Mf* scale, where the high ability high achieving group obtained the highest score. The relationship between measured personality characteristics and academic achievement becomes increasingly apparent but nevertheless remains ambiguous.

Allen (2) reported the relationships between the MMPI and the Edwards Personal Preference Schedule for 82 male and 48 female undergraduate students from the University of Miami. The two personality inventories were fairly independent of one another, but some suggestive correlations were present. The highest correlation of $-.51$ was obtained between the *Pd* scale and the Nurturance Scale. Only 5 of the 630 correlations exceeded $.40$. Of particular interest was the matrix of correlations among the Edwards variables. These scales are quite independent one from the other.

Borislow (12) studied the fakability of the Edwards Personal Preference Schedule. Nineteen subjects took the schedule under standard conditions and two weeks later one-third again took the schedule under standard conditions. Another group was instructed to respond similarly to a perfect individual characterized by traits society considers highly desirable, and the third group was instructed to respond in terms of how they would like to be rather than how they actually were. The retest profiles for the two experimental groups were more different from their original profiles than were the retest profiles for the control group. The consistency score and the profile stability coefficient were not related to faking. In general, the study demonstrates that subjects can purposely influence their scores even on this schedule.

Isolated reports on other personality instruments have been reported. Singer & Steffire (90) administered the Mooney Problem Check List-Adult Form to a group of 149 veterans coming for counseling. The number of problems and the number of serious problems checked correlated significantly in some instances with scales of the Guilford-Zimmerman Temperament Survey, but none of these correlations was high. Of the 20 correlations, only four were above $.30$, and the highest was $-.42$. Although the authors concluded they had evidence supporting the usefulness of this instrument, the data do not warrant their conclusion.

Auble (4) studied the validity of the Heston Personal Adjustment Inven-

tory by comparing the scores of freshman women tested at entrance to college and ratings provided three months later by faculty members, dormitory heads, and upperclassmen. Correlations between scores and mean ratings ranged from .03 to .35; tests of statistical significance failed to demonstrate any relationships between the variables.

Krumboltz & Farquhar (59) presented a brief but competent report on the reliability and validity of McClelland's *n*-Achievement Test. Using 169 university students in a "how-to-study" class, a test-retest reliability of .26 was found after a nine-week interval. For their total group of students, no significant correlations were found between the achievement-need score and the other variables, which included the Opinion, Attitude, and Interest Survey, the Survey of Study Habits and Attitudes, the ACE Psychological Examination, and an achievement test in how-to-study. Krumboltz (57) also has provided a useful review of the research done on the measurement of achievement motivation.

Bass (5) has continued his interesting work with his proverbs personality test. Using three scales designed to measure conventional mores, hostility, and fear of failure, he presented results of a number of studies of the validity of the scores. Small but significant differences were found between salesmen and nonsalesmen, penitentiary inmates and noninmates, college students and high school students, and Southerners and non-Southerners. A few significant but very low correlations were found between these scores and the scores on other personality inventories. Significant predictive coefficients were not found.

An intriguing analysis of the meaning of ratings made during interviews is reported by Woodworth, Barron & MacKinnon (108), who factor analyzed ten ratings obtained in life history interviews with 100 Air Force Captains and then correlated each of the four obtained factors with *Q*-sort items, other ratings, test scores, and criterion measures. The four factors labeled drive for professional achievement, stability of present adjustment, personal scope and capacity for adjustment, and character structure and mode of adjustment correlated significantly with several other variables. No evidence was presented to suggest how these "validity" coefficients might have been influenced by the reliability of the interview ratings.

From the many studies of projective instruments reported during the past year, only a few, of particular relevance to counselors, will be mentioned. Clark (21), continuing a study initiated in 1951, devised a multiple-choice Rorschach check list to be used in predicting academic success. The original test was revised, given to a group of students in 1954, and for cross-validation purposes to another group of students in 1955. The Rorschach score was not significantly related to grades and did not contribute to the prediction of academic success. This study, as have most others, has failed to reproduce earlier results of Munroe. Wysocki (109) studied the preferences of nonhospitalized persons for Rorschach cards and found that preference seems to involve two factors, color and affective tone. Relatively consistent differences were found among different groups of subjects with

some differences related to intelligence, sex, and extroversion-introversion. Counselors interested in vocational interests as related to problems of likes and dislikes will find this report stimulating.

Jensen (49) compared Thematic Apperception Test (TAT) responses of boys whose aggressiveness led them into trouble in school, boys who found socially acceptable ways of expressing their aggression, and boys who lacked aggressive qualities. The aggressive bad group and the passive group did not differ in the amount of aggression shown in fantasy, but the aggressive good group had significantly less fantasy aggression than the aggressive bad group. It did not differ from the passive group. The aggressive bad group was significantly different from the other two groups to the extent to which it used sexual responses, responses of tabooed sexual behavior, tabooed language, and tabooed violence. Again, although many of these differences were statistically significant, they were not large enough to suggest that these TAT responses could be used for individual predictions.

A few new developments in personality measurement require brief mention. Goldman (40) constructed a leadership rating scale for identifying high school seniors and college students likely to become leaders. Both reliabilities and validities were promising. Berdie & Layton (10) published a new personality inventory, the Minnesota Counseling Inventory, designed primarily for use with high school students but also appropriate for college freshmen. This instrument is a lineal descendant of the MMPI and the Minnesota Personality Scale. Jones (51) published an authoritarian scale from which he attempted to remove political attitudes and for which are available submeasures related to rigidity, dependency, anxiety, and hostility.

PERSONALITY DYNAMICS AND PSYCHOPATHOLOGY

A few papers appeared of direct concern to counselors regarding not the measurement of personality characteristics but rather the types of behaviors themselves called personality. Two of these papers reported research aimed at the definition of terms used in discussing personality. Most important is the paper by Eilbert (32), who made use of the critical incident technique to define "emotionally immature" reactions to situations. This term, like many others, is used with a variety of meanings by counselors. If Eilbert's effort can be extended to other terms used in a similarly loose fashion, the counselor's referents should become much more clear. Eilbert collected 458 incidents judged to be immature reactions by 67 psychiatrists, psychologists, psychiatric social workers, occupational therapists, nurses, and corpsmen. Most of these were collected in an Army hospital, although a small proportion were gathered in nonmilitary institutions. A classification system was evolved which provided 51 categories in seven major areas: demonstrating tolerance for frustration or stress; handling anger; accepting the dictates of authority, necessity, and convention; demonstrating integration, self-sufficiency, and achievement; handling manipulative and provocative impulses; dealing with sexuality; and demonstrating judgment, reasoning, and understanding.

Buss & Gerjuoy (16), attacking the same broad problem, attempted to scale in terms of intensity and abnormality each of 18 sets of adjectives. The judges, 42 psychologists, agreed with one another in their scaling of the words and demonstrated the consistency with which these terms could be ordered. The authors' conclusion seems justified that "when the task is denotative and the personality dimensions are specified, many words in current usage have behavior referents clear enough for judges to agree in scaling them." Reeves & Goldman (80) reported the examination of an interesting hypothesis that maladjustment of high school students was associated with the discrepancy between externally measured social class level and the student's own perception of his social class level. They related discrepancies between measures of these two social class levels and scores on the SRA Youth Inventory and the Ohio Social Acceptance Scale (OSAS). No relationships between discrepancies and the SRA Youth Inventory scores were found, but in grades 9, 10, and 11, statistically significant relationships were found with the OSAS, suggesting that students whose self-perceived social level was congruent with their externally measured social level were more positively accepted on a sociometric instrument by their peers. The hypothesis that maladjustment is associated with discrepancies between internal and external measures of social class level warrants further exploration.

OCCUPATIONAL RESEARCH

The beginning counselor will find a quite complete summary of relevant occupational research in a new book by Hoppock (44). The United States Employment Service continues to provide valuable assistance to counselors as shown mainly by its recent publication, the third edition of the *Occupational Outlook Handbook* (107), which contains concise and meaningful descriptions of more than 500 different jobs. Another publication (42, 36) from that office gives estimates of worker trait requirements. Profiles are presented for each of 4000 jobs, essentially those jobs that appear in Part IV of the *Dictionary of Occupational Titles*. These profiles present the results based on ratings descriptive of the occupation in terms of 11 aptitudes, 12 temperament traits, 10 interest traits, six physical capacities, and seven working conditions.

Fine (35) compared the functional occupational classification used by the U. S. Employment Service and the Minnesota Occupational Rating Scales, and reported correlations ranging from .40 to .89. His results suggest that raters arrive at somewhat related but far from identical job descriptions when making use of the two different classification systems.

Remstad & Rothney (81) demonstrated that the results obtained in occupational research depend on the method used in classifying occupations. They studied the relationship between father's occupation and child's occupational choice, classifying their data first using the *Dictionary of Occupational Titles* classification, next the classification used in the Kuder Preference Record, and finally a classification system devised by one of the

authors. If one assumes that the three systems were used with equal reliability, one must be impressed by the variation that accompanies different classification systems.

Much attention has been given to social status and occupations. Kahl (52) provided a summary of the theory of class structure, summarizing most of the relevant sociological research. He discussed the development of the dimension of class, the studies of position and prestige, the role of income, and the phenomenon of occupational mobility. This book, surprisingly enough, contains very little psychological data, and the large amount of information derived from vocational interest measurement is not even mentioned.

Rose & Wall (83) extended the customary prestige study of occupations by conducting focused interviews with 68 Negro high school students who had been asked to rank 15 occupations. The interview results suggested that the explanations subjects gave of the rankings they made did not confirm the hypotheses that have been offered in previous studies to explain the rankings obtained from white subjects. These results emphasized the need to extend prestige and status studies with due consideration for cultural and subcultural group differences.

An increasing amount of information has been published concerning the psychological and sociological aspects of specific occupations. Danskin (27) summarized much of this sociological information for 28 different occupations. The Rosens' (84) psychological description of an occupation should be read by every counselor, not only to obtain information about the occupation itself, but also to discover how exciting occupational descriptions can be. The Rosens studied 21 union business agents, making use of the Minnesota Multiphasic Personality Inventory, an intelligence test, and other records. Intensive interviews covered the man's job, relations with management, relations with other labor officials, and activities concerning contract negotiations, grievance handling, and other union matters. The authors conclude, "This study provides some indications that personality data, such as those made available by using the MMPI, may be useful in giving definition to some of the qualities necessary to fulfill role demands satisfactorily."

Dunnette & England (30), as part of a project involving the development of vocational interest scales in four engineering and scientific specialties, constructed a check list for differentiating engineering jobs. A 24-item check list was administered to 417 engineers and scientists, and the four developed scoring scales placed successfully 86 per cent of the subjects in the appropriate category.

Brayfield & Marsh (15) administered to 50 young farmers the Differential Aptitude Tests, the Kuder Preference Record, the MMPI and the Brayfield-Rothe Job Satisfaction Index. Inspectors' ratings were available. Numerical ability correlated .36 and scientific interest correlated .40 with performance rating, and literary interest correlated -.28 with job satisfac-

tion. Four of the MMPI scales correlated with job satisfaction: L, K, Depression, and Social Introversion-Extroversion. The highest of these correlations was .37. Job satisfaction and job performance were uncorrelated, which is not surprising in the light of Brayfield & Crockett's (14) review of attitudes and performance.

Two comprehensive studies have appeared on the occupation of physician. *The Student-Physician* (67), a sociological study of medical education, reported some rather amateurish attempts to obtain further understanding of the vocational motivation of medical students. Surprisingly, the definitive work of Strong and Tucker was not mentioned. A more sophisticated approach is presented in the book by Gee & Cowles (38). This volume, containing the papers presented at a colloquium, is required reading for any counselor working with medical or premedical students and for persons responsible for the admission of students to medical schools.

A few papers dealt more generally with over-all problems of occupations and manpower. Paterson's (76) Bingham Memorial Lecture identifies many often overlooked sources of wasted talent. Using both relevant clinical data and extensive research data, much of which came from the Minnesota Employment Stabilization Research Institute, this distinguished psychologist discussed means for reducing this wastage. Fine (34) presented a research program which has as its aim the determination for many jobs of the relative importance of specific knowledge and skills, with particular attention to the predictions of transfers from one job to another. Becker & Carper (6) explored the use of interviews of graduate students in physiology, philosophy, and mechanical engineering to learn how persons internalize their occupational identifications. From these interviews, interesting hypotheses are presented concerning the entrance of individuals into occupations.

Dynes, Clarke & Dinitz (31) attempted to find which social-psychological factors within a given socioeconomic group differentiated individuals with high aspirations from those with low aspirations. They hypothesized that persons with high aspirations more often would have greater difficulty with interpersonal relations within the family. Questionnaires from 350 university students supported the hypothesis. For instance, of the persons with low aspirations, 24 per cent felt unwanted by their fathers, whereas among the subjects of high aspirations, 42 per cent felt unwanted. Studies such as this, considered along with the classical Friend and Haggard study, suggest the possible existence of a U- or perhaps even a W-shaped relationship between personal family adjustment and vocational success. Finally, two cultural studies were presented, one by Ross & Ross (85) and one by Berdie (8). The authors of the first tested about 175 persons in an Apache community, using the SRA nonverbal test, the Kuder Preference Record, the Flanagan Aptitude Classification Test, and three projective tests. Among other things, they found that the mean scores on the aptitude tests varied greatly from test to test, the lowest means being obtained on the arithmetic test and the

highest on the scales test. The second study compared the academic and vocational motivations of Australian high school students to those of American students.

COUNSELING PROCESSES

Weeks (104) analyzed 37 interviews with 20 high school boys by listening to the recordings while reading typed scripts. He rated each client's response on a five-point scale of expressed affect. Correlations between different raters ranged from .91 to .94, unusually high for this kind of situation. A correlation of .57 was obtained between level of affect and talk ratio, the latter consisting of the ratio between the amount of client talk and the total talk. The results indicated that the method of affect rating used here should prove to be a valuable tool for use in interview analysis. Strupp (97) compared two methods of therapy, client-centered and analytic, by contrasting two case histories. Five dimensions were used: type of therapeutic activity, dynamic focus, degree of inference, initiative, and therapeutic climate. Definitions were provided for all five of the dimensions, and the results revealed the expected differences between the two cases. Again, the main contribution of this study is its suggestion of useful dimensions for counseling analysis.

Thomas & Mayo (99) studied civilian counselors who assisted more than 3000 Marine corps recruits in selecting one of 11 military occupations. At the end of counseling, the counselors were asked to make a prediction for 1300 recruits and then these predictions were compared with the criterion data regarding success. The predictions and criterion data then were fed back to the counselors. With this added information, the counselors made similar predictions for a new group of counselees, and the effectiveness of prediction before knowing the accuracy of their work and after was compared. With the first group, 19 per cent of the cases were classified as major errors; after the knowledge of results, only 11 per cent were classified as major errors. The correct number of predictions increased from 32 to 38 per cent. Providing information to counselors regarding the accuracy of their predictions helped improve their ability to make such predictions.

A study by Gustad & Tuma (41) compared different methods of test introduction and test interpretation in counseling. They used pre- and post-counseling self-ratings of abilities and interests. Different methods of test introduction showed no significant differential effects on client learning, and clients who had most accurate information about themselves prior to counseling learned the most about themselves during counseling.

Another study comparing two methods of counseling was that of Sonne & Goldman (91). The upper, the lower, and the middle 10 per cent of 440 high school seniors selected on the basis of score on the F scale of authoritarian attitudes listened to recordings of two interviews, one a client-centered interview and the other an eclectic interview, and then responded to a questionnaire to indicate their preferences. The results, somewhat ambiguous, suggested that students with authoritarian attitudes tended to

prefer the eclectic interview. This raises the question as to the effect of the counselee's personality upon his differential response to a variety of counseling methods.

EVALUATION AND OUTCOMES

The number of studies concerned with the evaluation of outcomes of counseling is impressive. Froehlich (37) added to the list of studies which have used changes in self-ratings as a basis for evaluating counseling. His results demonstrated that this criterion was unrelated to age, sex, grade, intelligence, or motivation for counseling. The question about this criterion still remains as to how much depends on the counselee's learning the proper things to say as opposed to more relevant behavioral changes. Another methodological study is that of Carlson & Rothney (20), who compared the classifications made by judges of questionnaire responses from counselees. Judges were able to agree in classifying responses pertaining to factual information, the reliability coefficients ranging from .60 to .95. Reliability was less satisfactory for questions pertaining to nonfactual questions where the same kinds of responses tended to mean different things to the various judges.

Spivak (92) reported a brief study of the effects of telling seventh grade teachers about the kinds of problems checked by their pupils on the SRA Youth Inventory. A control group showed no significant reduction of problems upon retesting, whereas the experimental group, whose teachers had been informed of the problems, showed a significant drop in the number of problems. Although the number of cases was small here, and the experimental manipulation minimal, the approach was intriguing in its possibilities. Caplan (18) reported a somewhat more sophisticated study using "problem boys" in a junior high school. The experimental group of counseled students improved significantly more than the noncounseled controls. Whereas prior to counseling the experimental group had obtained an average of 2.4 poor grades, after counseling they obtained .9 such grades. The control group figures were 1.6 and 1.7 respectively.

Caravello (19) studied the postschool records of high school students, some of whom had been counseled only by teacher-counselors and some of whom had been counseled by a guidance specialist. The groups were divided at random. A control group of noncounseled individuals also was available. There was no difference in employment status, but a significantly larger number of counseled students were continuing their education. Almost one-third of the students counseled by the guidance specialist remembered this was helpful, as compared to less than 10 per cent of the teacher-counselor group. The students showed a marked preference for guidance from specialists.

Patterson (77) compared 69 counseled and 468 noncounseled students in an industrial school. The two groups were similar in age, education, and relevant test scores. The counseled and the noncounseled groups did not differ either on length of time they remained in school or on grades obtained.

The negative results obtained here raise the question as to how adequately most counselors are prepared for counseling students who plan to enter an industrial school.

Kiell (54) submitted a check list and sentence completion form to 452 college freshmen and used 200 returned forms to evaluate the effectiveness of a faculty counseling program. Most of the students perceived the faculty advisor as someone to assist in program planning, and although responses appeared to indicate general acceptance of the program on the part of the students, these responses could hardly be called enthusiastic.

The relationships between three different types of criteria of therapy were studied by Kelman & Parloff (53) who had 15 neurotic patients complete seven measures before and after 20 weeks of group therapy. Three of the seven measures showed significant change, but only one of the 21 intercorrelations between change scores was significant. Changes in therapy did not go together; improvement was not a unitary process. Information regarding the reliability and the validity of the specific instruments used was not given.

An intriguing book by Cumming & Cumming (26) reported an experiment done in a Canadian community to influence the attitudes of the inhabitants toward mental health. Contrary to expectations, the carefully worked out program actually resulted in the development of negative attitudes. The attempt to provide information about mental health, and particularly about mental patients, appeared to create anxiety to such an extent that many persons in the community were eager to discontinue the program. The implications for the counselor here are many and obvious.

Another type of evaluation inference can be obtained from a recent study (47) published by the United States Office of Education. In a sense, the fact that only 40 per cent of freshmen in the United States graduate four years later, with an additional 20 per cent graduating at some other time, is an indictment of counseling and guidance programs in American colleges, or perhaps is a sign of their nonexistence. Particularly notable, however, is the finding based upon questionnaires from students that of all things in the colleges, they reacted most adversely to the quality and quantity of counseling and advising they had received. Sixteen items on the questionnaire related to faculty services, and rating low were, "assistance from counselors on how-to-study techniques, assistance from instructors on how-to-study techniques, and services of my faculty advisor in helping me select my first term courses." To quote from the report, "Students were almost unanimous, regardless of their ability level or the type of institution in which they were enrolled, in expressing a low opinion of the performance of the Counseling, Guidance, and Orientation functions in higher education." These results suggest the need for a vast expansion of the types of evaluation studies just reviewed.

STUDIES FROM RELATED FIELDS

Much research of relevance for counseling psychologists can be found in fields other than psychology. One of the more relevant reports (58) of

educational research compared the relative effects of three different methods of teaching a how-to-study course to university students. Criteria were an achievement test, a self-rating of study skills, the n-Achievement Test, and two opinion-attitude questionnaires. An eclectic method of teaching influenced student motivation more than did instructor-centered or student-centered methods. Other differences were not significant. Trueblood (101) studied 1711 university students who were employed, one-fifth of the total student population in the school, and found that work while attending college as a full-time student did not adversely affect grade point average. Ability differences were not related to employment. An important but non-research paper by Rudin (87) discussed the problem of academic anti-intellectualism as a problem in student counseling. He presented a point of view not often enough seen. "The author's position is that the counselor, as part of the enterprise, has a responsibility to it and to the academic community. It is therefore incumbent on him to further the realization of its goals by discussing with the student his motivations and attempting either to fan what slight spark of intellectualism he finds or to strike the spark himself." A contribution from medicine is of concern to the counselor, *Mental Health in College and University* by Farnsworth (33). Reassuring is his statement, "Since so many of the treatment problems are relatively acute, the psychiatrist is pleasantly surprised to observe that evidences of psychopathology, which in other settings might be gravely disturbing, are in this special and usually favorable atmosphere more likely to represent transient phases of reaction to stress. The inborn strong tendencies toward normality and a kind of emotional homeostasis are very strong at this age." The volume perhaps is most useful to help the counselor increase the university administrators' and faculty members' understanding of mental health problems on the campus. Farnsworth defines the counselor's role almost exclusively in terms of academic advising, thus reflecting the college culture in which he has worked.

During the year counselors have shown increasing interest in problems of religion and values as they relate to higher education and counseling. Nelson (72) studied the religious attitudes in 1950 of 887 persons who as collegians took in 1936 the Thurstone attitude scales toward religion. The shifts in attitude during the 14 years tended in the direction of more belief in God as a reality and less toward Sunday observance. Religious attitudes held in college tended to persist for at least 14 years after college. Institutional patterns became less clear during that period, as did North and South differences. Attitude scores in college were related to overt behavior 14 years later as shown by church attendance and other behaviors. For instance, the correlation between Attitude Toward God and religious activities 14 years later was .42.

In spite of the increasing concern of a few counselors for problems of religion and values, little if any attention has been given to the implications that changing theological opinion and theory have for personality development and for the types of problems counselees will bring to counselors. Meehl (64) presented some stimulating ideas on the relationship between

mental health and religion, attempting to clarify the roles of religion and psychotherapy as they contribute to mental health. He warns that therapists, although they must be concerned sometimes with problems of religion and values, cannot function as pastors, and similarly, the work of the pastor is not the same as the work of the therapist.

Williamson (106) effectively demolished the assumption that counselors can function without reference to or without revealing their own values. The counselor must remain a part of the culture that created him and not only inevitably will but should incorporate his values into his counseling.

A few studies appeared that provided descriptive information about specified populations. Leopold (61) surveyed the 1956 job status of about 3000 women graduates of 1955 from 108 colleges. Average salaries for women graduating with different undergraduate majors ranged from \$2660 to \$3670, with the lowest average salary being earned by art majors, the highest average salary by majors in the physical sciences. Well over 80 per cent of the women graduates expressed substantial satisfaction with their first jobs and about the same number judged that their first jobs were related to their undergraduate majors. Young (110) presented data on younger and older workers in seven different labor market areas and reported that patterns of unemployment were not related to age. Job seekers over 45 years of age tended to possess no higher job qualifications than did younger job seekers. The main difference between the younger and the older job seekers was in education. The results suggested that the handicap of age was not as great for job seekers as has been frequently presented. Condon (24) reported a ten-year survey of physically handicapped college students. He found that these students were employed in a wide variety of jobs, and in each occupation were found persons with many different kinds of handicaps. With only a few exceptions the physically handicapped students were receiving salaries similar to those that would be paid to persons without physical impairments.

Several recent reports concerned the development of counseling as a profession. Weitz (105) surveyed the staffing, organizing, administrating, and financing of research activities in student personnel work in 28 agencies in 10 different universities. In the counseling centers and mental hygiene clinics, he reported three stages in the development of research programs. The first stage involved research done by individual staff members with no direct university support. The second stage added occasional efforts by groups of staff members with formal support from the university. The third stage involved staff members whose primary function was research with direct financial support provided by the university. Research projects ranged from routine administrative research, such as is involved in establishing test norms, to the investigation of fundamental problems of human behavior, such as studies of the influence of emotional factors upon academic success. McCully (63) described the development of the Veterans Administration counseling program since its establishment in the early 1940's, with encouraging comment upon the increasing professionalization in this service. Albee & Dickey (1) presented an analysis of manpower problems in the

three mental health professions of psychiatry, psychology, and social work. Unfortunately, they paid no attention to the mental health function of the counseling psychologist on the college and university campus, although a large proportion of the work of this sort with college students is done by counselors.

Thrush (100) presented a case study of the occupational opportunity service at Ohio State University and showed how the staff of that center from 1952 to 1956 reformulated its own point of view toward counseling, shifting away from an emphasis upon vocational counseling to an emphasis on counseling for personal adjustment. One must assume that the need for vocational counseling continues and therefore must question who is providing this needed service. Rosse & Peters (86) studied the requirements and qualifications for vocational rehabilitation counselors in 51 agencies, reporting great diversity in the requirements both in terms of education and types of specialized experience. Meltzer (65) presented a discussion of the problems of the professional psychologist in private practice. Among other things, he raised some cogent questions concerning the motives underlying the development of professional codes of ethics. Rutledge (88) presented a description of certain aspects of marriage counseling, a specialty within counseling becoming increasingly popular.

Five papers have appeared in the *Personnel and Guidance Journal* describing counseling and guidance programs in other countries. Counseling in Belgium, according to Ostlund (75), differs from that in the United States in terms of theory, practice, and culture. Belgian counselors place great emphasis upon physique, wear white coats, and test with a great variety of instruments and apparatus. A picture of a rapidly expanding vocational guidance program in France was presented by Ostlund also (74). Australian counselors, according to Berdie (8), assist children in making vocational and educational decisions at an earlier age than do American counselors, and guidance tends to be more specific there than in the United States. We continue to learn more about guidance and counseling in Japan from the articles of Masuda (62) and Nishigaki (73).

CONCLUSION

The limited space for such a review has allowed only brief mention of a small portion of all the research articles which have relevance for counselors. This survey of the past year's research activities has led the reviewer to the conclusion that current research directly related to counseling, when compared to that done eight or nine years ago, shows greater sophistication, involves more persons, covers a much broader variety of problems, and tends to provide information of more immediate usefulness to counselors. The type of research that is most difficult in counseling, process research that studies the interactions occurring during the counseling interview, challenges many, but only a few persons now have the resources to conduct such research. Research that relates the characteristics of counselors, counselees, counseling process, and counseling outcomes remains the most

needed type of research. If the present trends can serve as a basis for prediction, the next ten years will produce a satisfying amount of such research.

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BOOKS AND REVIEWS

The outstanding event of the year was the appearance of Polyak's monumental *The Vertebrate Visual System* (122). Like *The Retina* (1941), it was immediately hailed as a classic. It contains the life work of a very learned anatomist, and the staggering range of information from history to anthropology defies review. It is likely that the section on the anatomy and histology of the visual system will be most often consulted, since here Polyak's contribution is truly exceptional. The historical section is fascinating. Also fascinating is the last part of the book in which Polyak painstakingly relates many ancillary branches of visual science, although the usefulness of this more philosophical section may not be universally acclaimed. Indeed, Wald has already strongly taken exception to it (165).

The books of Ronchi (133) and Le Grand (96) have been well translated. Ronchi's rather personal work is a praiseworthy attempt to unite all aspects of vision, but he treats the process of seeing with an unabashed mentalism that will annoy many. Le Grand's reference book, bursting with data, tables, and graphs, is close to a necessity.

Specialized books include Dartnall's (37) authoritative work on bleaching and regeneration of photosensitive pigments in vertebrates, Linschoten's (102) very complete account of depth perception, and Bartley's (8) attempt at the difficult task of producing a treatment of perception for beginning students. The field of aviation medicine has been enriched by Whiteside's fine book (174) on the problems of vision encountered by the high-altitude, high-speed flyer.

The work of summarizing the vast visual literature was bravely undertaken by Brindley (22), Links (101), Prentice (123), and Riggs (127), who have served physiologists, psychologists, and ophthalmologists well. Their areas overlap, yet all should be consulted by those who would survey vision the easy way.

PHOTORECEPTORS

The value of the photoelectric densitometer developed by Rushton and his collaborators to observe photochemical reactions in living eyes has again been demonstrated by Lewis (99, 100) and Rushton himself (136). Lewis

¹ The survey of the literature pertaining to this review was completed in April, 1958.

² This chapter was prepared under Contract NOrd 7386 between the U. S. Navy, Bureau of Ordnance, and The Johns Hopkins University.

followed the regeneration rate of visual purple in the rat during bleaching and in the dark. He cautiously concluded that bleaching with light of 517 or 469 $m\mu$ did not affect regeneration rate. Earlier, Hubbard had found that photoisomerization of retinene was important for the regeneration of visual purple in laboratory preparations. Rushton used his technique to demonstrate that isomerization was unimportant for this purpose in the living eye. In another place, Hubbard & Kropf (78) have again pointed out that light chiefly acts on rhodopsin to isomerize neo-*b* retinene to all-*trans* retinene, thus basing vision on a mechanism whereby light directly affects the shape of a protein chromophore.

Wald (163) has a good review of his work up to 1956, and recent studies include a variety of photochemical systems. Foremost is Wald & Brown's (166) analysis of human rhodopsin *in vitro*, showing a maximum in the absorption spectrum (λ_{max}) at 493 $m\mu$. This is low, except for some deep-sea fishes, and lower than in the living eye.

Wald, Brown & Brown (167) continue the ecological interest shown in the pigments of deep-water fish. An examination of fish caught at various depths showed a rough correlation between depth of habitat and a shift of λ_{max} toward the blue. In even deeper catches to 380 fathoms, Munz (111) observed values of λ_{max} as low as 478 $m\mu$, but reported an absence of systematic trend with depth. Carlisle & Denton (31) observed that the eye of the fresh-water eel contained porphyropsin and rhodopsin, whereas the mature animal ready for deep-sea migration, contained the golden pigment characteristic of deep-sea fish. This type of change with maturity was studied in detail by Wald (164) for the sea lamprey.

Other studies from Wald's laboratory were on the squid (79), alligator (168), and the bee, where a start was made by Goldsmith (56) on uncovering the almost unknown chemical mechanisms of insect vision. One pigment was extracted, but there may be others, since behavioral evidence indicates that bees possess color vision.

Intimations of biochemical complexity in single carp cones were found by Hanaoka & Fujimoto (70). Absorption spectra of 3 μ diameter spots in cone outer segments were measured with a microspectrophotometer. Different cones were discovered to contain different photopigments as shown by the λ_{max} positions. Most clearly identified were rhodopsin, porphyropsin, iodopsin, and cyanopsin, and several cones were noticed that appeared to contain two pigments.

Barer (2) reported a method of making accurate measurements of the refractive indexes of living cells that adds new interest to the role played by the geometry of rods and cones in the trapping of light by total internal reflection. R. L. Sidman, in Barer's laboratory, found refractive indexes of cone and rod outer segments of 1.387 and 1.405, respectively. O'Brien (114), in his theoretical treatment, took 1.022 as a calculated value of the relative refractive index of the cone outer segment to the surrounding tissue fluid. Barer estimates that this should be about 1.030, in excellent agreement with

O'Brien's value. A further consequence of having known-values of the refractive index of cones results from considerations of dispersion and wavelength. If cone angles and refractive indexes vary slightly, a filter mechanism becomes possible that may divide the spectrum into trapped light of different wavelengths. Barer states an interesting difficulty: Is light trapped in the classical three components, or is it trapped continuously?

PUPILLARY RESPONSE

Pupillary research has taken a cybernetic turn in Bleichert's (17) argument that the pupillary reaction is a widely integrated biological regulatory process, not just a simple reflex. In this he draws formal support from Stark & Sherman (150), and Stegemann (151). Stark & Sherman showed the pupil response to be a servosystem of low gain and great stability, and suggested that their technique might be applicable to the light reflex systems of persons who, due to pathology, exhibit oscillating pupils. The predictive value of their formulation was successfully checked by Stark & Cornsweet (149) using cases from studies in which the frequency of sustained oscillations of the pupil had been measured.

Drischel (44) described a detailed statistical analysis of the reactions of the pupil that followed a 0.01 sec. flash of light. Photoelectrically recorded pupillograms of 493 normal subjects were found to have an average latency of 230 msec. The minimum size was reached about 484 msec. after the pupil began to close and was nearly open again at 1,281 msec. The form of the reaction was asymmetrical, with the pupil narrowing more rapidly than opening.

Population data remain to be collected on the relationship between pupil size and light intensity. Seitz (143), however, had about 25 cases in each of eight groups between 5 and 80 years of age. Measurements were taken only in the light and the dark to get the range of pupil change.

Schweitzer & Bouman (141) have measured the differential intensity threshold of the pupillary reaction. For a background of constant luminance, the reflex was completely determined by the energy content of the flash.

VISUAL ACUITY

Numerous calculations have been made of the depth of field of the normal eye, but Campbell (30) has measured this directly. His criterion was the blurring of a 10 min. of arc black disc seen against a bright surround, a test claimed more sensitive than the direct measurement of acuity. Depth of field was found to be affected by those factors that alter acuity; i.e., luminance, contrast, and wavelength. For this reason no absolute values can be given without specifying the conditions of measurement, a matter nicely resolved by the good tables and figures.

Acuity measured at low luminance levels in the dark-adapted eye revealed interacting roles of rods and cones. In Brown & Woodward's (24) ex-

periment grating test objects were exposed in short flashes. Threshold luminance for a wide range of acuities and spectrally different illuminants showed that fine detail and the longer wavelengths required primarily cone activity, but there were many other combinations of detail and illuminance that depended on both rods and cones.

Acuity in normal eyes rose with longer exposure times according to Zanen & Klassen-Nenquin (181), and Sédan, Jayle & Farnarier (142) reported that binocular acuity was about 12 per cent better than monocular.

Acuity, as the separation threshold of two small luminous circles, was measured extrafoveally to 70° in the dark by Oliva & Aguilar (118), and related to the distribution of sensory units in the retina. Two-point acuity fell toward the periphery in a way to suggest that it was inversely proportional to the linear density of the sensory units.

The optokinetic nystagmus test of acuity is an interesting interaction between two different visual mechanisms that deserves more systematic attention, but has been little studied except in Europe. Gorman, Cogan & Gellis (58) noted its usefulness in testing children, and formal instrumentation was evaluated by Lewinski (98), Sachsenweger (137), and Günther, Noteboom & Plötz (68) with generally favorable results.

Acuity in the use of mounted telescopes and field glasses was measured in a very careful study by Köhler & Leinhos (90). The greatest gain in acuity with magnification came under light-adapted conditions, but it was also shown to be significant down to about 0.0001 m. Below this value, acuity decreased with magnification. Weiss (173) measured visual acuity with hand-held binoculars under field conditions. His results suggested that increasing magnification beyond 7-power, without the use of a rest, was of doubtful value.

BINOCULAR VISION

Portions of Linschoten's (102) extended treatment of the Panum-effect have already begun to draw hostile fire. Roelofs (129) presents a basis for refuting Linschoten's view of double localization of monocular stimuli under conditions of normal binocular viewing, and Wilde (176) took issue with him on certain geometrical arrangements in which disparity is impossible, yet which elicit the perception of depth.

Gilliot's (52) important work on aniseikonia with the Ames space eikonometer has been collected in a monograph. Chapters not earlier reviewed (127) concern how aniseikonia effects stereopsis measures taken by the Howard-Dolman method, the effect of lowered illumination of one eye on the measurement of stereoscopic depth, and a discussion of how these matters relate to the Pulfrich-effect. The finding that unequal luminosities of the two retinal images caused the longitudinal horopter curve to move nearer the eye with the least light is interesting. It is particularly so since Ogle & Groch (116) reported that unequal luminosities had no observable effect on carefully measured stereopsis. This study was done to put to rest

the ghost of Verhoeff's (1933) claim that such an effect did exist. Verhoeff, if not the ghost, is very much alive, as will be appreciated by his spirited reply (161).

The basic geometrical principles of binocular space according to Luneburg (1948) were set forth in a much simplified model by Blank (16), whose account will be welcome to those who were overwhelmed by the original presentation. A more specific part of this problem was treated by Shipley (144) in a study of the convergence function, using the Hillebrand alley.

Gogel (53, 54) and Gogel, Hartman & Harker (55) reported on a program to examine mathematically and experimentally the cues to perceived depth. When it was found that the retinal size of a familiar object did not determine absolute distance perception when other cues to depth were absent, attention was directed to the relationship between the perceived frontal extent (height or width) of an object in the fronto-parallel plane, perceived depth, and retinal disparity. A multiple was derived from a study of frontal size that translated binocular disparity into perceived depth. Also studied was an observer constant that related for a given individual the amount of disparity taken as equal to a corresponding amount of frontal extent.

The effect of exposure time on the stereoscopic threshold was measured by Ogle & Weil (117). The angular disparity threshold was described as an exponential function of the exposure time of the form $t = t_0 E^{-a}$, where t is angular disparity, t_0 is the threshold for 1 sec. exposure, E is time, and a is the rate at which the threshold increases with a decrease of exposure time. For three subjects, a was about 0.3. The contrast of the test object had little effect as long as the stimulus could be seen.

VISUAL THRESHOLDS AND BRIGHTNESS DISCRIMINATION

The quantum theory of vision continues to receive deserved attention. Baumgardt (10) evaluated the several quantum explanations of what occurs at the absolute threshold, and has stressed the inadequacy of the photochemical theory, even when buttressed by neural speculations, as an explanation for dark adaptation, area effects, and contrast phenomena. Jones (83) has been concerned with quantum efficiency of vision at scotopic and photopic levels covering nine \log_{10} units of adapting luminance. From an ingenious experiment on flicker thresholds and detailed calculations, he concluded that the maximum quantum efficiency of the eye is about 10 per cent, and remains high over a relatively wide range of adapting luminance.

Van den Brink & Bouman (160) applied the quantum theory to account for contrast thresholds of moving point sources of light measured under various conditions of velocity, exposure time, wavelength, and background brightness, and Bouman & Walraven (19) found that two quanta were needed to see a light as red, whereas three were needed for green.

Nolan (112) proposed a way of treating threshold data involving area, stimulus duration, and stimulus intensity in which energy, rather than luminance, is plotted versus time or visual angle. The data of Karn, of Rouse,

of Graham & Bartlett, and of Austin were analyzed and replotted with interesting results. Rouse's conclusions were reversed, and from the other data certain hypotheses resulted: where the Bunson-Roscoe law holds, retinal energy density is a constant; beyond the critical duration, where the law no longer holds, the retinal illuminance is a constant.

The distribution of scotopic sensitivity along horizontal and vertical meridians of the retina from 2.5° to 30° was measured by Zigler & Wolf (182), and the influence of a parafoveal surround on the brightness threshold of the fovea was investigated by Schubert (139), who concluded that the contrast effect is entirely a function of the photopic system. Contrast thresholds were also measured in assessing the effect of retinal image motion on vision by Krauskopf (92), who stabilized the retinal image optically to eliminate normal motions and introduced known motions experimentally. He found that sinusoidal motions below 10 c.p.s. lowered contrast thresholds, whereas frequencies between 20 and 50 c.p.s., raised the thresholds. In either case, peak-to-peak amplitudes of 1 min. of arc, or larger, were required.

In Barlow's (3) important paper a signal-to-noise discrimination hypothesis about the visual threshold was tested. The "noise" is from two sources, but the effect is the same. In the case of the absolute threshold, the limiting factor was fluctuations in the noise in the visual pathways as manifested by the dark light of the eye, and for the differential threshold it was the quantum fluctuations of the background light. The hypothesis that human thresholds are "statistical judgments of constant fallibility," with noise the random factor that produces errors, was upheld for incremental stimuli of small size and short duration. Departures from prediction, found for large stimuli and long durations, were considered in another paper that demonstrated complex relationships between temporal and spatial summation, and background intensity (4). Measurements of the dark light suggest a value equal to about 0.002 scotopic trolands. Barlow comes close here to abolishing the distinction between the absolute and differential threshold, but the validity of going as far as this was questioned by Baumgardt (11).

Stevens (152, 153) recently drew together his views on the measurement of perceptual continua, and with Galanter (154) has discussed in detail a number of sensory scales, including brightness. Here scaling methods resulted in a ratio scale that was a power function of the stimulus magnitude; i.e., subjective brightness was proportional to the luminance raised to a power, usually about 0.35. However, in his reply to Hunt (80), Hopkinson (75) has expressed disappointment in Stevens' wholehearted acceptance of the power relation. For the light-adapted condition, Hopkinson found $M = kL$ to hold, where M is the magnitude of the brightness sensation and L is the corresponding physical luminance. Only for the dark-adapted condition was the exponent used: $M = kL^{0.3}$.

The difficult problem of assessing the effect of stray light in the eye has

been materially aided by DeMott & Boynton's (39, 40) direct measurements carried out on the freshly excised eyes of steers and cats.

ADAPTATION

Barlow, FitzHugh & Kuffler (5, 6) have come to grips, at the neurophysiological level, with some of the problems of adaptation. Their experiments are important to psychologists because they attempt to correlate psychophysical and physiological data. The big discrepancy between the final levels of sensitivity reached after dark adaptation in the undisturbed eye and those measured by electrophysiological methods was overcome by the use of microelectrodes and carefully prepared cats' eyes. The dark-adaptation function recorded from isolated ganglion cells was followed through the rod and cone branches for 4 \log_{10} units. The absolute thresholds reached were still not as low as for the intact eye, but low enough to make untenable the claim for central mechanisms in dark adaptation. More complex visual mechanisms, in particular the area-threshold effect, were also followed electrophysiologically in isolated cells. Data relating area to light intensity at threshold for dark- and light-adapted eyes showed surprising changes in the organization of the "on" and "off" units of the receptor fields that suggested the operation of lateral inhibition mechanisms and a possible relation to contrast effects. This may be the kind of problem in which Ratliff and Mueller's (125) technique of neural synthesis would be useful.

While the above data support the lack of a central nervous role in dark adaptation, Granger & Franks (62) have pointed out some interesting relations with conditioning. The suggestion of central involvement is based on some low but positive correlations which they cautiously interpret, as well as physiological data from other sources.

Smith & Dimmick (146) repeated Thompson's (1949) and Haig's (1941) studies on the recovery of visual sensitivity following various degrees of light adaptation. The results were in agreement with Thompson's finding that light adaptation is not a process that is complete in a few minutes.

An interesting adaptation process neglected for 25 years was examined by Clarke (33): a steadily fixated test patch in the periphery rapidly fades in brightness and ultimately disappears. A modified Wright colorimeter was used to measure changes in perceived brightness of test stimuli with time. Data obtained extrafoveally to 40° with several wavelengths and luminances showed a latency following exposure of from 1 to 10 sec. that increased with excentricity, but was unaffected by wavelength or intensity. The delay was followed by a rapid adaptation (fading) that increased with excentricity. The relationship between this work and the effects of stabilizing the retinal image is obvious, but Clarke maintains that the delay and fading are not accounted for by a stabilization hypothesis, and the matter is left open.

FLICKER

The healthy impact of an engineering approach to visual research was

first seen in Rose (135) and, above, in Jones (83). Since 1951, de Lange (38) has examined critical fusion frequency (CFF) phenomena measured with sinusoidally modulated light, and his work is now found in a formidable monograph extending his analysis to higher luminance levels, impulse-shaped light modulation, and the effect of monochromatic light. The essence of the matter lies in the application of a new kind of measurement to the visual system, wherein an "attenuation characteristic" is found. This is the ratio of the output amplitude to the input amplitude as a function of the input frequency of sinusoidal light variations. The attenuation characteristic is arrived at by determining the CFF at each of a number of ripple ratios, defined as the amplitude of the fundamental divided by the average luminance. The condition for the CFF is taken by de Lange as the reduction of any ripple ratio by attenuation to some critical value, due to a threshold mechanism in the visual system. The ramifications are such that only the monograph will serve the interested reader, but one general finding from this stimulating work should be borne in mind. This is de Lange's conclusion that since the ripple ratio determines the CFF over a wide range, one cannot make physiological inferences from the light-dark ratio, as has so often been done.

Wilkinson (177) investigated the relationship between the intensity, frequency, and number of flashes in determining the CFF, and found that the frequency at which pulse trains fused increased with the number of pulses in the train, but was not related to intensity. The results are held to support Bartley's neural "alternation of response" theory, which Bartley, Paczewitz & Valsi (9) further investigated by manipulating the light-dark ratio in a study of brightness enhancement. In this experiment a pulse-to-cycle fraction of 0.3 and a frequency of 10.6 c.p.s. produced the most enhancement, although Bartley elsewhere discusses other important factors (7).

An experiment that measured the critical chromatic fusion frequency (CCFF) for pairs of colors alternated under carefully controlled conditions was reported by Truss (158). The results showed that persistence (1/c.p.s.) for both CFF and CCFF was a linear function of luminance on logarithmic coordinates. A high correlation was also found between persistence (1/CCFF) and the logarithm of the chromatic separation of the color pairs.

As a source of discomfort, flicker is of importance to illumination and television engineers. Collins & Hopkinson (35) determined thresholds for just perceptible, just obvious, just uncomfortable, and just intolerable flicker for stimulation of the whole visual field, and a surface of 20 by 30 degrees. Subject variability was great, and the curve relating flicker frequency to the probability of perceiving various kinds of flicker will be useful to workers in the applied areas of vision. The desensitization effect of strong flicker was confirmed, but at moderate light levels the effect was not great enough to guarantee adaptation when one enters a flickering environment.

Landahl (95) has now modified a previous model of a neural net to include the effects of area and intensity on CFF. On a less theoretical level, Weale (171) observed that the Ferry-Porter law is valid for retinal areas that may vary by a factor of over 30,000.

Schwarz (140) reported a potential application to heterochromatic photometry for the fact that the flicker rates of two lights can be accurately matched, and Clausen & Vanderbilt (34) have confirmed and extended Schwartz' (1938) measurements on beats due to combined photic and electrical inputs to the eye. The perceived beat frequency was the difference between the stimulus frequencies, and was unaffected by intensity.

The production of color perceptions by intermittent excitation, classically demonstrated by Benham's top, recurrently excites interest. Roelofs & Zeeman (130, 131, 132) have published a lengthy account of their observations on the importance of contour to perceived color that, regrettably, is largely qualitative. Perhaps some completely fresh analytical approach, such as Gehrcke's (48), would be more profitable.

ELECTRICAL STIMULATION

The telling blow delivered by Riggs, Cornsweet & Lewis (128) to electrostimulation as the method of choice for revealing the secrets of the eye has had no visible effect on the new papers from Motokawa's laboratory. The use of the electrically aroused phosphene covered a wide range of topics, although the emphasis has shifted, somewhat, from experiments primarily concerned with basic visual mechanisms to those showing interactions with other physiological processes. In the basic mechanism category were three papers on retinal induction. Motokawa & Akita (106) have mainly reviewed previous work. Onodera (119) compared measurements of the size of indirect retinal induction (simultaneous contrast) at the fovea and at the periphery. The induction effect was greatest at 10°, wavelengths in the red and green operated best near the fovea, and yellow and blue were most effective in the periphery. Motokawa & Komatsu (108) described a remarkable experiment in which the propagation of retinal induction in the retina was blocked and reflected by another retinal excitation process, such as a white line lying in the path of the induction wave. Indeed, the reflecting properties of a local excitatory process obeyed the laws of geometric optics, so that when the stimulus giving rise to the propagated front was placed at the focus of a parabolic white line image on the retina, the direction of propagation upon reflection was parallel to the axis of the parabola!

However astonishing this may be, Michaels (104) reports that he has confirmed the spread of excitation in the retina around the borders of two circles. This, to my knowledge, is the first support Motokawa has received from an independent research. It is dimmed, however, by Howarth & Treisman's (76) failure to find any effect of light stimulation on the electrical thresholds.

Also to be included in the category concerning the study of basic mech-

anisms were measurements of the electrical strength-duration function in rods reported by Yonemura & Nango (180), and a further investigation of the rods carried out by Motokawa & Aizawa (105) on two congenitally color-blind subjects.

Papers in which factors influencing the electrical thresholds predominate included three: by Wake (162), Kohata, Wake & Hamada (89), and Kohata (88). Pressures exerted on the eyeball decreased electrical excitability, abolished the supernormal excitability effects caused by colored light, and made it impossible to elicit the retinal induction effect. Kameyama (85) found that stimulation of the semicircular canals was clearly reflected in the electrical thresholds, Umetsu & Suzuki (159) reported an increase in the current required to elicit a phosphene during menstruation, and, according to Suzuki *et al.* (155), the thresholds were altered by drugs.

Two final applications were the most remarkable of all. Kobayashi (87) stated that the physical strength of tuberculosis convalescents was adequately assessed by the technique developed by Suzuki (1950), and Motokawa *et al.* (107) claimed that the effect of ionizing radiation could be detected in amounts of as little as one milliroentgen by measuring the subject's capacity to discriminate electrical phosphenes!

ELECTRORETINOGRAPHY

The experimental work on the electroretinogram (ERG) in men and animals has continued at last year's brisk rate. Bornschein & Goodman (18) stretched out the *a*-wave by high-speed recording, and observed in normal eyes two negative waves that were separated by a hump and followed by additional humps on the positive-going part of the response. The positive humps were nearly absent in a totally color-blind subject, and completely lacking in a congenitally night-blind person. Since the latencies of the first *a*-wave were the same in both normal and defective cases, latency alone does not distinguish the scotopic and photopic components of the ERG. Goodman & Bornschein (57) found only scotopic activity in *a*- and *b*-waves of the totally color-deficient subject. In the night-blind subject, the *x*-wave showed photopic activity, and the *a*-wave showed both scotopic and photopic response.

Heck & Rendahl (73), using a flash frequency of 1 c.p.s., reported data that were similar, in certain respects, to the above (18). The ERG in normal eyes also showed two *a*-wave components, with four positive ripples on the rising front of the *b*-wave. In normal subjects, preadaptation to light of 650 m μ suppressed the third positive wave, and adaptation to 549 m μ suppressed the fourth wave. The protanope's ERG was the same as that obtained on a normal eye adapted to red light. The ERG of the deuteranope was normal except when adapted to green light, whereupon the fourth wave was enhanced. Two totally color-blind subjects gave only the scotopic response.

Henkes (74) used selective amplification of the flicker ERG to reveal

cone activity, and Heck (71) analyzed the ERG obtained at varying flash rates. At low rates the ERG was polyphasic, becoming gradually diphasic as the flash rate was increased. At fusion the ERG consisted only of an *a*-wave and an off-effect that was studied further in another paper (72), and related to the operation of color-specific components of the response. That the off-effect in the ERG is observable only under light-adapted conditions was reported by Best & Bohnen (13, 14), who found that it merged with the on-effect after a short latency, combining to produce a single positive wave. It was also observed that at a frequency of about 40 c.p.s. an extended stimulus field of high intensity caused the ERG to respond as a train of positive waves, alternating markedly in potential. This effect was explained on the basis of the time relations between components of the ERG wave form.

Mahneke (103) separated two short flashes by intervals of 1 msec. to 2 sec. and recorded the ERG for light- and dark-adapted human eyes. The ERG responses following each flash in the light-adapted eye were similar until the dark interval was 25 to 50 msec. Then the flashes looked fused to the subject, and ERG responses of the same type could not be seen in the record. In the dark-adapted eye, the ERGs became indistinguishable at 50 to 100 msec., and subjective fusion was at about 50 msec.

In cats and frogs, the ERG does not rise in the dark following light adaptation until about 50 per cent of the rhodopsin has been regenerated. This has been confirmed in man by Elenius & Heck (45) using Rushton's *in vivo* method.

Brindley (20) has shown that additivity holds in the excised frog's retina for fields as small as 0.08 mm². The absence of interaction between neighboring parts of the retina in producing the ERG strongly supports the belief that the ERG comes wholly from the rods and cones.

The several papers of Dodt & Walther have shown: First, that the spectral sensitivity curve of the dark-adapted, lensless cat's eye as measured by both the ERG and recordings from isolated ganglion cells, was the same as the absorption curve for rhodopsin (41). Second, when flickering light was used to separate scotopic and photopic components of the cat's ERG, scotopic components predominated at flash rates of 10 to 20 c.p.s., but from 30 c.p.s. the spectral sensitivity of the photopic response was readily obtained (42). Finally, differences were measured in the scotopic spectral sensitivity curves of pigmented and albino rabbits that were attributed to the spectral reflectivity of the blood in the retina (43).

The origin of the ERG in isolated preparations of the frog's retina was studied in a series of researches from Leipzig. Pilz, Sickel & Birke (120, 121) used multiple electrodes of 0.1 to 0.3 mm. diameter and a small spot of light to measure the source and spread of the ERG. The retina was demonstrated to be an extremely uniform source of electrical potentials that was unaffected by either light intensity or adaptation. Sickel, Bauereisen & Lippmann (145) raised both atmospheric pressure and oxygen tension while recording the ERG from the frog retina and found that oxygen pressure in

excess of 1 atm. acted as a specific poison in lowering the height of the *b*-wave. Reduced oxygen tension, on the other hand, was shown by Brown, Hill & Burke (23) to have a specific effect on the *x*-wave in normal eyes following stimulation by red light where a fall to 50 per cent of the initial amplitude was noted. The amplitude of the *b*-wave following blue and white light was less affected, and the *a*-wave changed little under any illuminant.

NEURAL RELATIONS

The most exciting report last year was Svaetichin's claim of electrically recording three distinct response patterns from single cones of fish (127, 156). The recordings revealed not only cones having response maxima in different parts of the spectrum, but two kinds of cones that showed changes in polarity associated with wavelength. This work is of great importance to the Hering theory, but any verification this year has escaped my notice. Meanwhile Motokawa, Oikawa & Tasaki (110), proceeding from Svaetichin's earlier work (1953) on negative cone action potentials, have apparently discovered independently an element in the carp's retina giving rise to positive and negative potentials depending on wavelength. These were called A-elements, as distinguished from N-elements that give only negative potentials. The amplitudes recorded from N-elements, however, also varied with wavelength. The examples of recordings from A-elements do not agree very well with Svaetichin's records, as the response maxima and change-over points occur at different wavelengths. No inferences were drawn for the Hering theory. The polarity changes revealed in the A-elements were referred to "on-off" processes. From an analysis of the behavior of the N-elements, however, which show maxima at 470, 550, 600, and 650 $m\mu$, it was concluded that the carp retina contains four kinds of photopic receptors. Grüsser (64) found only negative potentials in cats when making intracellular recordings from cones and rods stimulated with flashes of different durations and flicker. He looked for the polarity effects, although he did not expect to find them with his incomplete stimulus arrangement.

The broad significance of the power of microelectrode work was well stated in the review by Jung, Creutzfeldt & Grüsser (84), where an appropriate amount of attention was given to visual mechanisms in a noteworthy effort to correlate neural activity with such psychophysiological matters as critical fusion frequency, the Brücke-Bartley effect, and contrast phenomena. The most recent attempt of these workers to relate the reactions of isolated retinal elements to selected psychophysical data are found in Grüsser & Kapp (65) and Grüsser & Rabelo (66). They interpreted their data as a superimposed excitation-inhibition mechanism in the synapses, and developed hypotheses for handling, as an example, after-image phenomena.

The mainspring of the work of Barlow, FitzHugh & Kuffler (5, 6) is that sensory thresholds may properly be considered as signal-to-noise detection problems. This concept was carried further by FitzHugh's (47) statistical analysis of single ganglion cell discharges elicited by short flashes

of light near the threshold. Following each near-threshold flash, the discharge pattern was photographed and the number of discharges was counted and used as an index of response. The rate of transfer of information by the ganglion cell was calculated from this index on the analogy that "the analysis of a nerve fiber message by the brain is similar to the engineering problem of detection of a signal in a noisy communications channel." The calculated information rates indicated that the sensitivity of this method of detecting signals was satisfactory. The "noise" part of the problem was studied by Kuffler, FitzHugh & Barlow (94), who measured continuous discharges from the ganglion cell, both in darkness and with steady illumination. The statistical properties of the fluctuating maintained discharge were shown in a distribution of the intervals between impulses. In the range of 1 to 50 msec., a skewed curve of the gamma type resulted.

Sustained activity was also found at the cortical level by Köhler & O'Connell (91). What they call "quasi-steady currents" can be recorded at all times from the visual cortex of the cat, except during sleep.

COLOR VISION

Trichromatic concepts in vision antedating Young and Helmholtz were carried back to Mariotte (1620-1684), Lomonosov (1756), and Palmer (ca. 1780) in a fine essay by Weale (172).³ Brindley (21) is recommended for an excellent technical interpretation of the basic data and hypotheses underlying trichromacy, and, to clarify the Hering side of the issue, Hurvich & Jameson (81) have brought together their quantitative work supporting the opponent-colors theory. Included is a brief discussion of neurophysiology, where the Hering theory has always been weak. Doubtless the writers will watch closely the fates of Svaetichin and Motokawa.

The first Edridge-Green Lecture, delivered by Wright (178), was a thorough critique of diagnostic testing. After his own costly colorimeter, Wright favors the still expensive Nagel anomaloscope, which fails only for tritanopia. Richter's (126) modification of the Nagel removes the limitation, and the cost problem is solved by Weale (170) and Walls (169), who describe anomaloscopes built with filters.

New data were published on some persistent problems in color vision. For 20 years it has been objected that the luminances of the primaries in color mixture equations do not add linearly. The conclusion from Wienke's (175) careful experiments was that Abney's law does not hold, and that units of luminance vary in size throughout the spectrum. Another problem has been to get light enough in the blue end of the spectrum to determine properly the hue discrimination function. Bedford & Wyszecki (12), using a xenon arc and fields down to 1.5 min., found a minimum in the $\Delta\lambda$ curve at 420 m μ . Small fields yielded the same functions as large, except that $\Delta\lambda$ was larger. A third problem concerns the exact shape of the spectral sensi-

³ Those who like background will also delight in Charen's (32) note, tracing the language of vision in the Indo-European tongues to the ancient root AK.

tivity curve, generally treated as smooth with a single peak at about 550 $m\mu$, but found in some studies to be a complex function with several maxima. Humps in the blue and the orange, with possibly some between, have now been described by Sperling & Hsia (147), Hsia & Graham (77), and Graham & Hsia (60, 61). Sperling & Hsia laid the objection that the one at 470 $m\mu$ was due to the yellow macular pigment. The hump at about 600 $m\mu$ was confirmed, and the consensus was that the peaks represent fundamental processes in cone activity. Kinney (86) also measured the spectral sensitivity curves to confirm and extend her previous data measured at the scotopic threshold. Again the humps appeared.

Spectral luminosity curves play a leading part in Graham & Hsia's analysis of color defects (60, 61, 77). While many agree that protanopia is due to the loss of the red receptor system, few have accepted deuteranopia as a lower sensitivity of the green receptors. New evidence (77) was given for a loss in spectral sensitivity at the longer wavelengths in six protanopes, and for a similar loss at the shorter wavelengths in five out of six deuteranopes. The matter was settled conclusively by data obtained on a rare unilaterally color-blind woman whose right eye was normal and whose left eye was deuteranopic (60). The left eye showed a sensitivity loss compared to the right eye in the range of 430 to 625 $m\mu$. Data on flicker fusion, color matching, hue discrimination, and color mixture also revealed characteristic differences between the two eyes. A third paper (61) summarized this most complete and valuable set of measurements.

At a far more delicate level, differences between the color sensitivity of the two eyes is a matter of practical concern in making color matches. For specifying such differences in standard terms, Burnham (28) developed equations that predict from a few measurements inter-eye differences for any color. Burnham, Clark & Newhall (29) have also shown that, while there is a time-order effect in color matching, matching precision was unaffected by stimulus position. Regarding the age of the color matcher, however, Gilbert (51) found that a steady decline set in after the twenties, although variability was great at all ages from 10 to 90.

VISUAL PERCEPTION

Systematic writing, found lacking last year (123), again flourishes. Bruner (27) has offered a neural model for perception; Bevan (15), after relating perception theory to general behavior theory, found an unsound or nonexistent basis for future development; the Gestalt position was supported by Zuckerman & Rock (183); and Bartley (8) wrote a lengthy introduction at the student level. Graham's (59) solid analysis is to my taste. His argument against the need for retaining both terms, "sensation" and "perception" is recommendation enough. Finally, a philosopher has vigorously damned all psychologists in perception. Hamlyn's (69) attack makes interesting reading, is grossly unfair, and will probably not draw a formal reply.

Over 30 papers on form discrimination appear in the report of the 1957 Tuft's symposium (179), and not all are restricted to applied problems. In particular, the informational aspects of form received emphasis. Additional work is seen in Krullee (93), who constructed and verified hypotheses for predicting the thresholds for form discrimination, and Attneave (1), who attempted to tie down what subjects judge to constitute complexity in a form. He found that the number of turns in a shape and its symmetry accounted for about 82 per cent of the total variance in judged complexity.

Basic to the problem of form is how a contour arises in the first place. This is difficult enough in the simple eye of *Limulus*, where Ratliff & Hartline (124) have suggested a basis for border discrimination in the mutually inhibitory effects found in discharge patterns when neighboring receptor units were illuminated. In the Mach bands, however, contours are seen even when a true edge in the retinal intensity distribution is lacking. O'Brien's (115) analysis stems from the fact that the retinal mosaic has finite spacing, and that to counteract the essentially Gaussian filter characteristic of the eye to white light it might be preferable to use a second-difference correction to describe the process of contour enhancement, rather than the second spatial derivative correction used by Mach, Ludvigh, and others. Observations with rotating discs indicated this to be a profitable hypothesis, and evidence from Ronchi & Toraldo di Francia (134), suggesting that the Mach bands depend on the cones, lend it some support. Fiorentini & Ercoles (46) measured the spatial and temporal gradients at the limit of visibility of the bands, and interpreted their data in terms of neural events. Finally, Greene (63) found, in a reanalysis of some of Ludvigh's data, basis for supporting a hypothesis of inhibitory mechanisms in adjoining retinal areas as explaining certain aspects of the bands.

Size discrimination was investigated by Jenkins (82), who showed that stimuli at the close range of 2 ft. were consistently found larger when matched to objects at 20 ft. Gulick & Stake (67) noted that size constancy was adversely affected if the exposure time was reduced to 0.1 sec. or less, and size and shape constancy were investigated by Leibowitz, Bussey & McGuire (97), who compared test objects with photographs of such objects. An important finding was that the mechanisms underlying size and shape constancy must be different, for shape constancy was reduced by only 50 per cent in the reproductions, whereas size constancy disappeared almost entirely. The fact of higher constancy for size and shape with binocular viewing was confirmed, but little difference between the use of one or two eyes was expected for photographs. Nevertheless, the surprising result was a slight decrease for size.

Brown continues his work on the visual discrimination of motion with measurements of the upper speed thresholds, as influenced by extent of travel and luminance (25, 26). The relationships involving luminance were complex, but, in general, more light was required to see motion as the velocity of the moving object increased. Differential thresholds for motion were

reported by Notterman & Page (113), who confirmed Hick's (1950) findings for the discrimination of a step in an initial velocity. As initial velocity V increased, $\Delta V/V$ decreased to a minimum of about 9 per cent at 1° to 3° of visual angle per second, and then increased. Motion in a far less restricted sense was discussed in experimental and theoretical papers by Gibson & Gibson (49, 50), who have found that continuous perspective transformations seen as a shadow on a translucent screen are adequate stimuli for the perception of objects.

The work on the figural aftereffects was extensively treated in Spitz' (148) review of the current status of Köhler & Wallach's theory of cortical satiation, and Sagara & Oyama's (138) account of studies in Japan. Conklin (36) found that figural inspection had no influence on the autokinetic illusion, and concluded that since cortical satiation was not involved the movement must be due primarily to retinal events. Motokawa, Nakagawa & Kohata (109) related the aftereffects to a retinal induction mechanism, but the Köhler field theory of interacting electrochemical processes locates the seat of the aftereffects, and many other perceptual phenomena in the cortex. Thomas & Stewart (157) took an experimental look at this by attempting to alter cortical fields with a DC potential impressed across the head. A variety of perceptual tests made under this condition yielded the same results as were normally obtained. These data are certainly interesting, despite the uncertainty of what happens to the flow of current when electrodes are simply applied to the scalp.

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HEARING^{1,2}

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INTRODUCTION

The past year has produced a vast body of publications in hearing. Articles have appeared in acoustical journals; in medical, engineering, physiological, and physical journals; and, in fact, almost everywhere except in psychological journals. The titles are too numerous even to list here, so we must limit severely the area covered by the present review. This is made easier by the fact that many excellent reviews of special topics have appeared recently.

Jerger (64) has reviewed the subject of audiology very fully and has also presented (65) a rather thorough treatment of auditory adaptation. Hughes & Rosenblith (59) present electrophysiological evidence on auditory sensitization, and Epstein & Schubert (30) study reversible auditory fatigue. Glorig *et al.* (40) provide a statistical treatment of the data from the Wisconsin State Fair Hearing Survey.

Growing out of the work of the Armed Forces—National Research Council Committee on Hearing and Bio-Acoustics (CHABA)—there has appeared a series of papers summarizing much of their research activity. Davis & Usher (24) discuss zero hearing loss, and, in a CHABA symposium, Usher (121), Johnson (66), Hoople (56), Hirsh (55), Carhart (15), Webster (125), Glorig (41), and the CHABA Council (16) report on various phases of military audiometry. Another outcome of CHABA activity has been the issuance of the Air Force Regulation No. 160-3 on hazardous noise exposure (1).

For the most part the present review will be a description of experiments, with some of their backgrounds, which relate most directly to auditory theory—theory in the broad sense advocated by Rosenblith (104). It will consider the single ear first, the cochlea, way station responses, cortical responses, and psychophysiological findings, and then will discuss some of the additional phenomena which appear when the two ears operate together.

THE COCHLEA

Three reviews, Davis (23), Galambos (36), and Tasaki (117), and a paper by Békésy (2) provide the richest yield of information about the

¹ The survey of the literature pertaining to this review was completed in April, 1958.

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cochlea and its mechanical, chemical, electrical, and neural activity to be found in the literature. In addition to these, there have been numerous papers on rather more specific topics.

Borghesan (10), Goodhill & Holcomb (45), Spyropoulos (113), and Naftalin & Harrison (95) have been concerned with the fluids of the cochlea, their circulation, viscosity and pressure, and their effects on cochlear response. Eldredge, Covell & Davis (29), and Lawrence & Yantis (75) have investigated the recovery of the guinea pig ear from acoustic trauma. Covell & Rogers (21) and Pestalozza *et al.* (98) have studied pathological changes and the decreased bio-electric potentials in the ear of senile guinea pigs. Mizukoshi, Konishi & Nakamura (93), Borghesan (9), Nakamura (96), Falb -Hansen *et al.* (31), and Fern ndez & Brenman (33) have investigated various pathological effects of toxic agents, acoustic trauma, oxygen deprivation, and lesions on cochlear function.

Wever, Vernon & Lawrence (127) have added the monkey to the list of animals whose cochlear potentials have been studied. They find a frequency range from 100 to 15,000 c.p.s. The potentials they observed were smaller than in other mammals studied; but while the monkey's ear was less sensitive, it was harder to overload.

COCHLEAR FUNCTION

B k s y (2) has shown that the four principal theories of cochlear action—resonance, telephone, standing wave, and traveling wave—are each possible with differences only in the stiffness and the coupling between adjacent portions of the basilar membrane, but he adduces so much evidence for the last that we are not left much choice. This evidence is strongly supported by Tasaki's (117) work on electrical potentials, and leads Tasaki to say, "The pattern of traveling waves in the cochlea demonstrated by the electrical method supports, beyond any reasonable doubt, the legitimacy of B k s y's arguments. . . ."

The steady-state condition for a particular frequency—and a two-cycle signal is enough to establish it—is that of vibration of the basilar membrane from the base up to a region determined by the frequency. From the maximum toward the apex there is a sharp drop in amplitude and then a reversal in the phase of the vibration, with one or more smaller reversals as we proceed further toward the apex. On the basis of this picture we might expect that all fibers from the base up to the maximum or beyond it would be innervated and that we should hear all pitches from the highest to that corresponding to the frequency of stimulation. This, of course, is not the case. We hear only the low pitch, and the auditory nerve "hears" only the low pitch. The apical fibers (plus some others) are firing, not all of the fibers from the base up. This is one of the most challenging facts about the cochlea, and B k s y abandoned the ear to go to the arm for an explanation.

BÉKÉSY'S MODEL

Békésy (3, 4) describes a large mechanical model of the cochlea. It is driven, over a range of two octaves, by an electromechanical transducer (the stirrup), and its basilar membrane extrudes along its length. It is several inches long, and straight instead of snail-like. The model's pattern of movement is similar to the ear's pattern of movement (traveling wave, in most of the experiments). It has a maximum near the "base" for high frequencies, and near the "apex" for low. When the model is pressed against the skin one feels a sense of touch in a small region along the skin. As the frequency is changed, this spot moves but continues to feel small in extent, even though a considerable length of skin is actually being vibrated.

What Békésy has shown is that pitch apparently depends upon a boundary or contour effect, probably fundamentally like parallel phenomena in vision. Take Mach's rings as an example. Here we see a series of light and dark rings, not because the rings are there, but because of changes in the rate of lightness change. The boundaries are between areas where the rates are different, not the absolute lightness. Possibly, if we knew how the nervous system accomplished this in one sense modality we would understand them all. Huggins & Licklider (58) undertook to explain "sharpening" of neural excitation in the ear by means of a neural device which adds the negative second derivative of the excitation to the first derivative. This is a technique which has been employed by Kovasznay, Joseph & Newman (73) in experimental television to improve the sharpness of fuzzy pictures, and appears to be well within the capabilities of the nervous system. Possibly the model should be extended to other modalities.

EFFERENT PATHS

One of the exciting recent findings is that the olivocochlear bundle (bundle of Oort) has a function—suppression. Rasmussen (102) studied its path and found it to be an efferent bundle, originating in the olivary complex and terminating in the cochlea. Since then, Galambos (37, 38) has shown that the fibers end near or upon the inner hair cells. He finds that stimulating a region near the midline on the floor of the medulla, rostral to the obex, reduces neural responses to clicks being presented to the animal's ear. This tract, or higher efferent tracts reported by Rasmussen, must be the basis for the finding reported by Hernández-Péon, Scherrer & Jouvet (53) that the response of the cochlear nucleus to auditory stimuli in the unanesthetized cat is suppressed when the cat "attends" to a live mouse or to an interesting smell. The same effect is observed when the cat's forepaw is given a mild shock.

It appears likely that some of these efferents constitute the mechanism (or one of them) for auditory inattention—a phenomenon which is beginning to attract considerable interest from audiologists. At a recent International Conference on Audiology held at the Central Institute for the

Deaf, there was considerable emphasis upon the subject of "listening." Along these general lines, Rowland (106) has investigated electroencephalographic (EEG) responses to conditioned auditory stimuli in connection with arousal by "important" sounds, and sleeping through "unimportant." He presents evidence to show the role of the reticular formation in such phenomena.

NONLINEARITY

Aural harmonics, difference tones, summation tones, Tartini tones, best beats, the residue, periodicity pitch, the missing fundamental, and remote masking are a group of phenomena, closely related in that they all depend upon some kind of nonlinearity, probably in the inner ear.

Aural harmonics and combination tones.—The classical explanation (Helmholtz) of aural harmonics and combination tones is that the ear distorts at high intensities—that displacement is not a simple linear function of the applied force. It is common to approximate this nonlinear relationship by means of a power series into which is substituted the expression for the applied force. Now if we consider a force which is the sum of two sinusoids having frequencies f_1 and f_2 and substitute this in the equation, we obtain a series of terms. The first term is a steady pressure (dc). The second term (linear) contains f_1 and f_2 , our original tones. The third term (square) gives us $2f_1$ and $2f_2$ (aural harmonics), $f_1 - f_2$ (the difference tone) and $f_1 + f_2$ (the summation tone). Higher powers than the second add still further to the richness of distortion products. The difference tone and the summation tone arising from the square term are equal in amplitude, and this fact has caused considerable unhappiness. We hear difference tones readily but hear summation tones with difficulty, even when they fall in more sensitive parts of the hearing range. Tartini heard the difference tones from his violin and employed them in tuning it, but did not notice the summation tones. They were discovered a century later by Helmholtz.

Meyer (88, 89, 90) argues that Tartini tones do not arise from harmonic distortion, but instead from the movement of the "phragma" which he likened to the behavior of a leather chair seat (nonlinear, if anything ever was).

The evidence for distortion is incontrovertible if one accepts cochlear microphonics as having anything to do with hearing (Meyer does not), and this fact is pointed out by Lawrence & Yantis (74) in a discussion of aural harmonics and best beats. The fray is also joined by Chocholle & Legoux (17, 18) who do not deny aural harmonics, but argue that the method of best beats is not valid for measuring them.

It appears to the reviewers that the question at the bottom of all of this is not whether the ear is nonlinear, but where and how? Does the ear square, cube, etc., as in a power series, or does it rectify? Let us examine other related phenomena.

Remote masking.—Bilger & Hirsh (5) discovered that a loud, narrow band of high-frequency noise produces uniform masking of low-frequency tones; i.e., the subject's audiogram is uniformly elevated by an amount which is a function of the level of the band of noise. Their interpretation was re-examined by Deatherage, Bilger & Eldredge (26), who conducted additional experiments with the conclusion that the phenomenon is due to a rectification occurring in the inner ear. This interpretation receives support from the earlier work of Deatherage, Davis & Eldredge (25), who recorded potentials from the first and third turns of the guinea pig's cochlea, and found evidence of three kinds of masking: the ordinary kind, the interference effect described by Wever & Vernon (126), and remote masking. They attribute the last to an unsymmetrical mechanical action of the cochlea, rectification.

Spieth (112) also has studied the downward spread of masking, using both tones and bands of noise. He attributes the phenomenon to combination tones which the overloaded ear produces, and has checked this interpretation using the method of best beats. With generator tones of 950 and 1050 c.p.s., for example, he finds difference tones of 100, 200, and 300 c.p.s. Where the generators have sound pressure levels of 100 db, the three difference tones have levels of 75, 63, and 61 db, respectively. Other frequency combinations show comparable results.

Missing fundamental, residue, periodicity pitch.—A long time ago Fletcher (34) reported that a loud complex tone with its fundamental missing (filtered out) has the same pitch as the normal tone. He attributed this phenomenon to distortion in the ear. The difference tones between adjacent harmonics have the same frequency as the fundamental, and add, to provide a substantial sound. Jeffress (60) observed that Fletcher's phonograph record illustrating this phenomenon works equally well at low levels where distortion would not occur. Jeffress undertook further examination of the phenomenon, using organ pipe tones, and found that when the fundamental is missing, the pitch of the fundamental is also missing. He attributed the disparity between his results and those of Fletcher to differences in the phase relations among the partials. The partials of Fletcher's tone generator were all in phase, and the difference tones would add, while the organ pipes' partials were in random phase. Fortunately no one read this study, and the search for the missing fundamental continued. Schouten (109) obtained low-pitched tones from a series of high frequencies when conditions were favorable. More recently Thurlow (120), following up work by Thurlow & Small (118) and by Small (111), has obtained low-pitched sound from a variety of interrupted high tones and from pulse trains. Huggins (57) got tones where there were no tones by binaural interaction of white noise, and Licklider (81) showed that the low tones heard as the result of high-frequency stimulation cannot be masked by low-frequency noise but can be by high. The inescapable conclusion is that the high-

frequency portions of the cochlea are capable of providing low-frequency information to the brain, and this conclusion is borne out by work on both sound localization and cortical potentials.

De Boer (8) reports that a phenomenon resembling Schouten's residue can be observed when a restricted series of equally spaced components are sounded, even when they are not harmonics of their difference frequency. Starting with the condition where they are harmonics of their difference frequency, and where the low pitch one hears is that of the fundamental, he raises the frequencies of the components and finds that, for small changes, the pitch rises also instead of remaining fixed at the difference frequency. He reports that the timbre depends strongly upon the relative phases of the components, and appears to be related to the shape of the envelope of the wave form. Groen & Versteegh (47) in a related study employ frequency modulation to investigate frequency analysis in complex wave forms.

Licklider (83), using a sixteen-harmonic tone, finds that changes in the phase relations among the harmonics produce discriminable differences in the pitch or in the timbre of the tone. The differences are greatest when changes are made among the high harmonics. Cramer & Licklider (22) also report obtaining distinct pitch from a train of pulses of random polarity.

Goldstein (42), Goldstein & Kiang (43), and Kiang & Goldstein (69), in a series of studies, have investigated the cortical responses to various sound stimuli: clicks, bursts of tone, and bursts of chopped noise. In the unanesthetized cat they detected cortical following up to nearly 200 pulses per sec. When the cat is anesthetized, the following-rate is considerably lower. They did not find tonotopic localization for the following-frequency, although they did for the tonal frequency when tone bursts were employed. For the bursts of chopped-noise and clicks, the amplitude of the cortical response is a function of pulse repetition rate. It is large for low rates and small for large. This leaves the question of the cortical representation of periodicity pitch, at least for frequencies above 200 pulses per sec., still unsolved.

Goldstein & Kiang (44) have also studied the synchronization of the stimulus, the N_1 response at the cochlea, and the associated cortical potential, using various stimuli presented with and without a masking noise. They find that an N_1 response is always accompanied by cortical response, but that under some stimulus conditions, e.g., where there is a masking noise and where the stimulus onset is gradual, the N_1 response is not discernible although the cortical response is. They offer a probability model to account for their results.

Whitfield (128) also stresses the difference between the cortical responses in the unanesthetized cat and the anesthetized, and finds, for amplitude modulated tones, representation at the cortex of the modulation frequency as well as the modulated frequency.

Vernier & Galambos (122) have studied the responses of single units of the medial geniculate body to repeated clicks. They find following at low rates and a gradual dropping off of response as the rate is increased.

Nonlinearity—where?—The foregoing makes it apparent that the inner ear is nonlinear, and, of course, the conversion from sinusoidal stimulation to nerve spikes is not a linear process. The question is, where does the nonlinearity occur? A part of modern theory conceives of the cochlea as resembling, in the way it functions, a series of narrow filters, each followed by a detector. A 500 c.p.s. tone would thus affect a limited number of nerve fibers centered at 500 c.p.s. But when we sound a loud 2000 c.p.s. tone and a loud 2500 c.p.s. tone, we also hear a 500 c.p.s. tone. Where did it come from? Are we distorting ahead of the filters so that there is actual sound energy being delivered to the 500 c.p.s. filter; or are we combining the neural outputs of the 2000 c.p.s. and the 2500 c.p.s. detectors so that they coincide 500 times per second? There appears to be evidence that both things can happen, most of the evidence for the latter coming from work on binaural hearing, to be considered later.

A short time ago Wood (129) observed that the unsymmetrical wave form obtained by partially clipping a sinusoid during its positive half-cycle and not clipping during its negative half-cycle sounds different depending on which side is up. That is, if the clipped half-cycle is associated with the inward movement of the eardrum, the experience differs from that associated with the reverse condition. Work on this phenomenon is being continued in the University of Texas Defense Research Laboratory at the present time, using the unsymmetrical wave form, $\sin x + \frac{1}{2} \sin (2x + \pi/2)$, and commutating connections to an earphone. Subjects can respond consistently about 95 per cent of the time, basing their judgements on differences of "pitch" or "quality," and learning to associate the sound for one condition with one response, and the sound for the other with a different response.

This difference due to asymmetry in the stimulus is to be expected from the asymmetry of the stimulation process itself; the hair cells are fired during only one half-cycle. We must conclude that, whatever other distortion may occur in the ear, the final detection process resembles that of a half-wave rectifier. A full-wave rectifier or a square-law detector would not be sensitive to asymmetry.

LOUDNESS AND PITCH MEASUREMENTS, AND PSYCHOPHYSICS

Stevens (115) has continued his work on the measurement of loudness. With Zwicker & Flottorp (131) he considers the role of the critical band in loudness summation, using both bands of noise and groups of tones. Later (116) he offers a revised equation for the calculation of the loudness of wide-band noise of known energy distribution. Pollack (100) continues his study of loudness, this time using interrupted white noise. Robinson &

Dadson (103) present data on the threshold of hearing and on equal loudness relations for pure tones, and compare their data with those of others. Burns & Hinchcliffe (11) compare thresholds determined by means of the Békésy audiometer with individual pure-tone audiograms, and find substantial agreement.

There have been several studies of pitch. Thurlow & Bernstein (119) investigate simultaneous two-pitch discrimination, König (72) studies the effect of time on pitch thresholds, and Michaels (91) studies difference limens for narrow bands of noise. In the field of musical pitch, Corso (20) studies the absolute judgements of tonality by college musicians.

The attempt to replace the threshold concept by decision theory concepts continues. Two letters-to-the-editor (85, 86) and an article by Green, Birdsall & Tanner (46) discuss the quantity, d' . The last employs it in a study of the relations among signal intensity, duration, and detection. For those interested in detection theory the reviewers recommend a paper by Marill (87), who has written with unusual lucidity on the subject.

Hamilton (49), using conventional masked thresholds, examines data from several studies relating signal intensity and duration for various bandwidths of the masking noise. Blodgett *et al.* (6) study the relation between signal intensity and duration for various interaural phase relations. They find again that the monaural condition is indistinguishable from the two homophasic binaural conditions, that uncorrelated noise (independent sources for the two ears) gives a somewhat lower threshold than the monaural, and that the various antiphase conditions give still lower. They find different slopes for different durations. Harris (51) studies peak vs. total energy in short tones, and finds evidence for a dependence on peak energy, but concludes that the important physical characteristic of the stimuli remains unknown.

Two studies of underwater hearing thresholds have appeared. Hamilton (50) finds underwater thresholds to be higher than in air by from 44 db at 250 c.p.s. up to 60 db at 4000 c.p.s. He attributes the findings partly to a change of the resonant frequency of the ear canal and partly to hearing by bone conduction at the higher frequencies. de Haan (48), in a study of hearing in the whale, incidentally investigates underwater hearing in man and finds a 60 db elevation of threshold. He argues that underwater hearing is by bone conduction, but, since his work did not extend below 1000 c.p.s., the possibility of hearing at low frequencies via the external canal, as suggested by Hamilton, still remains. This is given support by the fact that de Haan's thresholds at 1000 c.p.s. are higher when the external canal is air-filled than when it is water-filled.

THE AUDITORY CORTEX

In addition to the studies of the cortex mentioned earlier in connection with frequency-following, there have been several studies of the role of the

cortex in other aspects of hearing (binaural cortical phenomena will be discussed later). Butler, Diamond & Neff (12) find that the cat, after bilateral ablation of auditory areas A I, A II, Ep, and somatic Area II, cannot only relearn a frequency discrimination problem but can perform nearly as well for near-threshold differences as before the operation. However, Diamond & Neff (28) find that the ability to learn to discriminate tonal patterns (melodies) is lost with complete ablation of A I, A II, and Ep. Destruction of A I alone, following learning of the pattern discrimination, leaves the habit intact. Destruction of A I and parts of A II and Ep destroys the habit, but it can be relearned. It requires total destruction of all three areas to destroy both the habit and the possibility of relearning it.

BINAURAL PHENOMENA

Two important new functions appear when we examine binaural listening: the ability to localize sounds, and the ability to hear what we want to listen to in the presence of turmoil.

Localization.—Ever since Lord Rayleigh gave it his blessing, interaural time- or phase-difference has been recognized as one basis for sound localization. The other is interaural intensity-difference. The first really modern study of localization was that made by Stevens & Newman (114), who found the accuracy of localization to be good at low frequencies and at high, but poor in the range from about 1500 to 5000 c.p.s. A great deal of work has been done since, and the past year has produced many titles. Some of the experiments have employed loudspeakers as sources and some have employed earphones.

Several studies have attempted to assess the roles of intensity and time in localization. Feddersen *et al.* (32) study the localization of high-frequency tones and find that for the inaccurate region from 1500 to 5000 c.p.s. subjects require a larger intensity difference than is provided in nature for a given lateral position of the sound source. The source is always localized nearer to the median plane than it actually is. They include measurements of interaural intensity-differences as a function of frequency and azimuth position of the source, and a set of measurements of interaural time differences as a function of azimuth. The last agree remarkably well with Woodworth's table (130) based on a rigid sphere.

Mills (92) in a very thorough study of sound localization reports thresholds, measured at the median plane, of about 10 μ s. for time and of about 0.5 db for intensity. These are for the optimal frequencies, 500 to 750 c.p.s. for time difference, and 3000 to 6000 c.p.s. for intensity. The 10 μ s. threshold corresponds to about 1° of azimuth. When the source is moved toward one side, the thresholds become much larger. Mills presents curves for all combinations of his parameters: time, phase angle, azimuth angle, frequency, and intensity difference.

Most localization studies involving earphones have followed Shaxby &

Gage (110) in balancing intensity against time, asking the subject to adjust one or the other to bring the sound back to the middle. This year has seen several such studies. One of these by Kikuchi (70) involves commutating the sound to the two ears by means of an ingenious mechanical switch. Death-rage & Hirsh (27), using clicks, find a linear relation between the logarithms of the interaural time differences and the intensity differences expressed in decibels. Leakey & Cherry (76) find that the intensity-time balance can be seriously upset by the addition of noise.

Moushegian & Jeffress (94) employ an earphone technique which does not require balancing time and intensity, and therefore permits using a greater range of intensity- and time-differences. Using earphones, they match the location of a tone having various combinations of intensity- and time-differences with a noise whose interaural time-difference the subject can adjust. The two stimuli, the tone and the comparison noise, are presented alternately. Results with four subjects for a 500 c.p.s. tone can be expressed by a single equation relating azimuth position with time- and intensity-differences, but the constants in the equation varied widely from subject to subject. One subject showed great resistance to the effect of intensity-difference and depended almost wholly on differences of time. The others employed combinations of intensity and time. The large individual differences found here may explain the wide discrepancies in the time-intensity equivalences reported in the literature.

Leakey, Sayers & Cherry (78) report binaural fusion under interesting conditions. The stimuli to the two ears are different carriers, 4 kc. to one ear and 4.1 or 4.2 kc. to the other. The two carriers are modulated by the same signal, a low frequency tone or band of noise. In all cases the subjects experience a localizable sound associated with the low frequency.

Recent interest in stereophonic sound reproduction has led to considerable work involving the use of two loudspeakers. Kock & Hanson (71) and Leakey (77) rediscovered several phenomena described earlier by Sandel *et al.* (107), which result from the use of two loudspeakers in free field. Leakey, however, added a new observation of considerable interest. He found that when a subject listens to a loudspeaker, but wears loosely a pair of earphones, he hears the sound from the speaker as inside his head. If he moves his head through a small angle, this perception continues, but, if he makes large head movements, the sound now appears to be "out there." Apparently the observation of Wallach's (123) that kinesthetic cues are important in externalizing sound perception is borne out here.

The best treatment of the problem of stereophonic sound reproduction which the reviewers have seen is that of Clark, Dutton & Vanderlyn (19), who consider not only the intensity relations involved, but also the usually neglected problem of obtaining lifelike interaural time differences.

Binaural masking phenomena.—Ever since the work of Licklider (79) and Hirsh (54), it has been known that the masked threshold is greatly

affected by reversing or shifting the interaural phase (or time) of a signal presented in a background of noise. Similar results can be obtained by shifts in the noise. The findings apply both to tonal signals and to speech. When the masker is speech having one interaural time relation, and the signal is speech having another, we have the "cocktail party effect." Pollack & Pickett (99) study this phenomenon, employing binaural tape recordings. A talker supplies the "message" (Harvard PB Monosyllabic Word lists) to both ears (in phase), and other talkers supply "babble" to each ear separately, one, two, four, or seven talkers per ear. The experimenters find that with two babblers, one to each ear, the threshold for 50 per cent intelligibility is about 12 db lower than for monaural listening. With more babblers the improvement is less; with seven, the difference is only about 5 db. The reviewers believe that if Pollack & Pickett had employed the same babble for both ears, but had introduced a time-delay (corresponding to a lateral displacement) in the channel to one ear, they would have achieved still greater differences between stereophonic listening and monaural. Pollack & Pickett employed uncorrelated sound for their babble; past experience indicates that larger differences would have occurred with correlated sound.

Along this line, there is an increasing interest in the use of binaural hearing aids to provide a basis for localization of sound, especially for the blind-deaf, and to provide further the added intelligibility that comes with binaural listening. The new transistorized aids with their microphones located near the ears make such listening possible. Carhart (14) discusses this new development.

Harris (52) in his chapter in the *Annual Review of Psychology* last year mentioned, but left to the next reviewer to discuss, a paper by Jeffress *et al.* (63) on masking phenomena. This is a theoretical paper which shows how two simple models (one monaural and one binaural) can account for many masking phenomena. The authors conduct several experiments suggested by the theory, and find good experimental support for it. The theory incorporates two hypothetical neural networks described earlier; one, involving essentially the same features, was described independently by Licklider (80) and by Jeffress (62). It is referred to by Licklider as an autocorrelation device. The other was described by Jeffress (61), and would now be called a cross-correlation device. Licklider (82) says that Wallach proposed essentially the same mechanism.

Sayers & Cherry (108) present both a mathematical treatment and an elegant drawing of such a mechanism. It incorporates both an autocorrelator and a cross-correlator. The authors say that their model has features in common with Licklider's (82) triplex theory of pitch, but to the present reviewers it appears more closely related to Jeffress' model in that it does not make provision for the separation of place-pitch and periodicity-pitch required by Licklider's theory.

The concepts autocorrelation and cross-correlation have been fashionable in hearing theory of late, and rightly so, but they can lead to confusion. When we think of a correlator, for example, as in a piece of laboratory equipment, we think of a device which actually computes a correlation coefficient (whether normalized or not). The neural networks described above, on the other hand, are better thought of as devices which determine a value of τ which will maximize the correlation. The nervous system is interested in what time delay produces the largest correlation, not in the magnitude of the correlation itself.

BINAURAL NEUROPHYSIOLOGY

Lower centers.—There have been several papers recently on neurophysiological interaction at various levels in the nervous system. Rosenzweig & Sutton (105) have studied interaction at the lateral lemniscus. They find that the neural response there to binaural stimulation is not simply the sum of the activities of the single ears. They conclude, therefore, that binaural interaction must have occurred at some level below that point.

From the standpoint of one of the reviewers (61) who wrote in 1948, "The existence of such a [speculative] mechanism would be strongly suggested if one could find a region . . . where impulses could be obtained with binaural stimulation under one phase condition and would drop out when the phase was shifted . . .", one of the most exciting papers of the year is that by Galambos (39). Galambos says:

When both ears are stimulated, some units are exquisitely sensitive to whether or not the sounds have been presented simultaneously. Time differences of one hundred microseconds between stimuli can clearly be preserved in terms of unit responses at some brain locations.

Galambos describes the frequency-place organization of the dorsal cochlear nucleus, and presents some data on the posteroventral and the anteroventral nuclei. He says that in the superior olive, single units have been isolated in all five major subdivisions which may be driven by either ear or both, and in the accessory segment he finds the units described above, which respond differentially to interaural time differences.

He concludes, in addition to the passage quoted above:

The basilar membrane is "unrolled" both anatomically and physiologically, at the various levels of the classical afferent pathway. A particular tone produces its effects at particular places in the brain.

The neural effects of each sound will include both excitation and inhibition, and every sound, whether it be tone, click, or noise, probably produces a unique pattern of excitation and inhibition in the cells of the brain.

Binaural cortical phenomena.—Several papers, including some on pathology and on audiology, have considered the role of the cortex in binaural hearing. Neff *et al.* (97) find that when the auditory cortex of the cat is

bilaterally extirpated, the cat's ability to localize sound is greatly impaired. They show that this impairment is auditory. The cat responds to the problem as though the situation is familiar, and makes visual responses with no deficit. Comparable lesions in nonauditory areas produce no degradation of the localization response.

Lombroso & Merlis (84) present evidence that in addition to the classical ectosylvian areas, the suprasylvian area must be considered as belonging at least in part to audition. Because of the difference of its response to ipsilateral and contralateral auditory stimulation, and because of its involvement in kinesthetic and motor activity, they believe it plays a role in the localization of sound.

Calearo (13) in a very interesting paper uses the phenomenon discovered by Bocca (7) to study brain-damaged patients. Bocca found that, when speech is presented dichotically with good quality, low-level speech presented to one ear and filtered speech to the other, subjects achieve higher articulation scores than when either one alone or both are presented to one ear. He employed, for the good quality speech, a level that yields articulation scores of about 30 per cent. The filtered speech obtained from a 500 c.p.s. low-pass filter yields less than 50 per cent articulation scores. When the two are presented to the same ear, the scores are no higher than 50 per cent, but when one is presented to one ear and the second to the other ear, the scores average around 80 per cent. Calearo employs this technique with patients having lesions in one auditory cortex, and finds that the scores are high or low depending upon which ear receives the unfiltered speech. If the low-level, unfiltered speech reaches the intact cortex (via the contralateral ear) and the loud, filtered speech reaches the injured cortex, the scores are as high as those for normal subjects. If, however, the reverse presentation is employed with the unfiltered, low level speech reaching the damaged area and the loud filtered speech the intact area, the scores drop to less than 50 per cent.

In view of the importance of binaural phenomena in revealing damage to auditory pathways at levels above the cochlea, it is reassuring to see the interest being displayed in these phenomena by clinicians. Studies of localization of sound are reported by Jongkees & Veer (67, 68) and by Walsh (124), studies of diplacusis by Fumeaux (35), and of the perception of binaural beats by Price *et al.* (101). One test, which has apparently not been employed in the clinic and which might well have diagnostic value, is the masking level difference [see, for example, Hirsh (54)] resulting from reversing the interaural phase of a tonal signal in noise. Normal subjects show differences of 10 to 15 db between the homophasic and the antiphasic conditions. The failure to show such a difference would be revealing if it occurred with audiograms which indicated hearing in the individual ears. It is also possible, according to the models we have discussed, that a patient could be deaf in one ear by audiometric tests, and still have

binaural hearing. These things can only be discovered by looking for them.

The program of the May meeting of the Acoustical Society of America arrived just as the reviewers were finishing this chapter. From the abstracts of the papers on hearing to be presented there, it looks as though the next reviewer will also have a rich crop of interesting material to harvest.

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PHYSIOLOGICAL PSYCHOLOGY^{1,2}

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This review deals principally with those studies which have in some way manipulated the brain in an attempt to discover how it is organized to serve behavior. Several such studies which have been reviewed recently elsewhere, notably those dealing with pharmacological agents and intracerebral self-stimulation, have not been included here.

NEW BOOKS

"This book . . . has a still more important purpose; namely, to teach the student to think in a new way about the living organism and the activity that we call behavior." Hebb (77) achieves this purpose in his *Textbook of Psychology* by weaving into a stimulating and intriguing exposition a widely varied content ranging from communication theory and social interaction to single-cell activity in the nervous system. On the conviction that psychology is fundamentally a biological science, he deals with social and applied psychology, as well as learning, perception, and emotion, from a biological point of view. He provides the student with just enough facts to lead him to reflect intelligently about the problems of psychology, avoiding belaboring the mere traditional. Only rarely is a textbook written to stimulate the learner to probe into the complexities of a subject; Hebb has written such a book.

The *Vertebrate Visual System* by Polyak (170) is a compendium of currently available knowledge about the visual system which, as Klüver writes in his foreword to the book, "will for generations to come be consulted by all investigators seeking information on the relations of vision to the retina, the visual pathways, and the subcortical and cortical visual centers, as well as by students interested in the structure and function of the central nervous system in general." Although this book deals primarily with the anatomy of the visual system, experimental findings and naturalistic observations relating to the function of these structures are not ignored. As a source book on the visual system, this book, with its detailed

¹ The survey of the literature included in this review was completed in May, 1958.

² The following abbreviations and symbols are used in this chapter: EEG (electroencephalogram); CR (conditioned response); CCR (conditioned cortical response); CAR (conditioned avoidance response); US (unconditioned stimulus); UR (unconditioned response); CS (conditioned stimulus); CER (conditioned emotional response); UCS (unconditioned cortical stimulus).

delineation of data, theoretical formulations with respect to function, and 1100 references, is unequaled in the field.

Equally useful to the investigator interested in the function of the visual system, as well as in the analysis of primate behavior in general, is the new impression of Klüver's (97) *Behavioral Mechanisms in Monkeys*. This monograph should find a wider audience now than it did when first issued in 1933, and should have particular appeal to the greatly increased number of investigators now attempting to assess the effects of pharmacological and other physiological agents on the monkey's behavior.

Of considerable interest to investigators in psychosomatic medicine, as well as to those interested in the conditioned reflex from a theoretical standpoint, is the book by Bykov (31) which is now available in a recent translation by Gantt. This book presents the results of many experiments demonstrating that kidneys, heart and blood vessels, respiratory apparatus, digestive tract, metabolism, heat exchange, periodicity of physiological functions, and thermoregulation can be conditioned by procedures similar to those developed by Pavlov and others. The book represents the "efforts of more than 100 investigators who under the leadership of Bykov worked out methods and conducted research to extend Pavlov's theories to the practical level of clinical medicine and psychiatry." However, the author's attempt to relate these processes to those of cerebral cortex is superficial. His analysis in neural terms is mainly a statement that "Pavlov recorded and proved the laws governing cortical processes in experiments on salivation" and therefore, by analogy, the same processes apply to conditioning of the viscera. No conclusive experiments appear to have been done to demonstrate whether or not these cortical processes exist. It is argued that they do in the way Pavlov said they do, and extrapolations are made to include the new findings. Thus, although this book is a useful source book of recent Russian conditioning experiments, it contributes little to an understanding of the cerebral processes in conditioning.

The results of the many studies on the effects of pharmacological agents have been brought together in several reports of symposia (60, 64, 85, 96, 232) and, more recently, in a very comprehensive review by Wikler (234). Of these, the report edited by Garattini & Ghetti (64) is outstanding for its penetrating analysis of the effects of drugs on many aspects of behavior.

BEHAVIORAL EFFECTS OF IRRADIATION

Several recent studies on the behavioral effects of irradiation have confirmed the earlier findings that not until the terminal stages following lethal doses does the problem-solving ability of adult animals deteriorate. Davis *et al.* (41), Harlow & Moon (73), Harlow, Schrier & Simons (74) and Riopelle, Grodsky & Ades (188) tested monkeys on a variety of problem-solving tasks known to reflect brain damage. The results are consistent in that no effect is apparent from either small or large, single or repeated doses of irradiation. In fact, there is, after the initial stages, more than

normal improvement. The improvement is generally attributed to the fact that irradiated animals are much less active, less aggressive, and more tame than normal and are thus more attentive in the testing situations. These studies agree, however, that although there is no deficit in problem solution, there is a decrease in activity level and a loss of persistence in carrying out the tasks.

These results on learning and activity are generally confirmed in the dog by the findings of DiMascio *et al.* (48). In this animal, too, there appears to be little effect of irradiation on problem-solving ability until the terminal stages of lethal doses. However, in dogs there appears to be a differential effect depending upon the accuracy of preradiation performance, the better performers being the more affected. With the dog, as with the monkey, activity level and general persistence in performance are decreased.

In the rat, Blair & Arnold (15) find that retention of maze learning is affected only in the first three days of irradiation. Blair (14) finds that even with very high doses directed at the adult rat's brain, acquisition of a multiple-T maze is superior, running times are faster and performance is less variable than in untreated animals. Fields (52), however, finds that irradiation affects initial learning on an elevated multiple-T maze and that retention may be affected if tested on sufficiently difficult mazes. Further evidence that the central nervous system may be affected is found in the study by Furchtgott, Echols & Openshaw (59) that relatively small doses administered during the gestation or neonatal period affect performance on the Lashley III maze when the rats are 50 days of age. Furthermore, of potentially great significance is the finding of Schwartzbaum *et al.* (199) that irradiation increases brain excitability as measured by decreased thresholds to electroconvulsive shocks.

TECHNICAL DEVELOPMENTS

Stereotaxic methods.—Recent improvements in stereotaxic instruments, atlases of subcortical structures, and the refinement of implanted electrodes and catheters are making subcortical structures increasingly accessible to investigators. The enthusiastic application of these technical developments has, however, frequently led to a disregard of the limitations of the stereotaxic method which were recognized by its originators, Horsley & Clarke (86), in 1908 and later by Olszewski (163). They emphasized that the coordinates for locating structures are at best approximations and subject to several sources of error. The major source of error, variability in the location of the structures relative to the coordinates, is beyond the control of the experimenter. Unless investigators supplement location of structures by set coordinates with x-ray and electrophysiological determinations, they must prepare many experimental animals in the hope that some placements will be in the intended location. Also, histological confirmation is, of course, always necessary. Other difficulties are inherent in this method. Intervening structures may be damaged considerably, the lesion itself is rarely well-

circumscribed, and adjacent blood vessels are injured which may damage tissue remote from the lesions. Confident statements about locus of lesions may therefore not be made without histological analysis of the whole brain. These limitations notwithstanding, the stereotaxic method is a valuable research tool.

As described by Wyss (237, 238), electrolytic lesions can be improved somewhat by using radio-frequency waves which result in a relatively more circumscribed lesion and relatively less damage to surrounding blood supply. Even more satisfactory, however, is the ultrasound method of producing lesions developed by Fry *et al.* (56). Recent studies by this group (7, 8, 9, 57, 89) indicate that such a lesion can be discretely placed, is well-circumscribed, and most importantly does not damage blood vessels either in the vicinity of the lesion or in the path of the beam. In addition, it is possible to regulate the frequency of the ultrasound so as selectively to damage fibers or cells. Recently, however, Bowsher (19) has shown that frequencies different from those used by Fry may have to be used to achieve these selective effects. Although the ultrasonic technique would appear to be almost ideal, it has the considerable disadvantage of requiring cumbersome and complicated devices, so is not, therefore, at the present time, a generally useful laboratory instrument.

Implanted electrodes and catheters.—Stimulating and recording through electrodes in unanesthetized preparations has proven most useful in studying the function of deep-lying cerebral structures. The techniques used with such success by Hess (82) have been made more generally useful by the development of multilead electrodes such as those of Delgado (42), more sturdy skull attachments by Sheatz (204), a simple precutaneous method of implantation by Lilly (112), and the development of a noninjurious wave form in the stimulating current by Lilly *et al.* (113). Recent improvements in microelectrode techniques as described by Hubel (88), Strumwasser (211), and Ricci, Doane & Jasper (185) make it possible now to study the relationships between single-unit activity and behavior. There are most significant developments which, together with recording through macroelectrodes, should provide physiological psychologists with the tools necessary for demonstrating the central processes involved in learning, said by Hilgard (84) to be one of the most pressing needs in psychology.

A critical question with respect to the use of implanted electrodes is whether or not the presence of the electrodes in the brain and the application of stimulating current permanently alter the tissue in the structures being studied. The many control studies on the effects on behavior of the implanted electrode *per se* suggest that any damage which is done is of little consequence to the behavior being studied. On the other hand, studies such as those of Delgado (44) and, more recently, of Collias & Manuelidis (39), which evaluate histological effects of implanted electrodes demonstrate that in individual preparations investigators must not ignore the possibility

of severe damage to structures not only in the path of the electrode, but in other tissue with common blood supply.

Caution must be exercised in inferring from the behavioral effects of electrical stimulation the normal function of the structures stimulated. Alonso-de Florida & Delgado (4) have shown that aggressive and docile behavior may be elicited from the same structure at different times during prolonged stimulation. Bower & Miller (18) have demonstrated a similar paradox with respect to the rewarding and aversive effects of stimulation. Sheer (205) has reviewed many studies which clearly emphasize that under some circumstances the effects of stimulation are clearly the same as the effects of a lesion, in other circumstances just as clearly the opposite. These apparent contradictions must be considered in describing the function of cerebral structures since, on the basis of electrical stimulation alone, the interpretation is equally tenable that the effects are the result of excitation, dysynchrony, inhibition, or the absence of activity. The way out of this paradox would seem to be to design stimulation experiments so as to include not only comparisons of the effects of stimulating several structures, but also comparisons of the effects of stimulating and of destroying the same structures.

In addition to electrocoagulation and electrical stimulation, investigators are using chemical stimulation to study the functions of subcortical structures. Miller (128, 130), in studies of motivation and learning, has had considerable success in adapting the earlier methods of Andersson (5) for injecting hypertonic NaCl solutions into the third ventricle. MacLean (115) has described a technique for use in unrestrained animals by which it is possible to stimulate the same point in the brain electrically or chemically and at the same time to record the EEG and behavioral changes. It is apparent from the results of this study that cerebral tissue greatly restricts the spread of soluble substances in the brain, making localization of function possible by chemical techniques in spite of widespread doubts to the contrary. In a recent study Olds & Olds (162) have adapted the methods of electrical self-stimulation to enable the self-injection of chemicals into the brain. With some chemicals they obtain "rewarding" effects similar to those obtained with electricity. However, Fisher (53), in describing his technique for studying maternal and sexual behavior of rats in response to intracranial chemical stimulation, was not able to induce the same behavior by electrical stimulation, suggesting that some tissue, for example hypothalamic, may be more responsive to chemical than to electrical stimulation. He does, however, like MacLean, emphasize the marked degree to which the chemicals are confined to the site of injection.

ELECTROPHYSIOLOGICAL CORRELATES OF BEHAVIOR

Almost forty years ago Pavlov had the temerity to suggest that when the salivary response is conditioned to an auditory stimulus, a temporary

connection is formed in the cerebral cortex between the auditory center and the food center. Even though the unequivocal demonstration that such a process does occur has probably been the most pressing need in physiological psychology, not until recently has there been a concerted attack on this problem. It began with the early demonstration that alpha blocking in humans has the properties of a conditioned response. With the application of recent technical developments to the study of this and other conditioned electrical responses in the brain, there now appears to be high promise of achieving an understanding of the neural events in learning.

Conditioned cerebral electrical responses.—A major breakthrough was achieved by Morrell & Jasper (145) when they conditioned cortical electrical responses in unanesthetized monkeys. These responses were of two forms: a repetitive discharge specific to the frequency of the unconditioned flickering light stimulus, and a localized activation pattern limited to the sensory receiving area for the US. Of particular significance in this work is the demonstration that cortical conditioning has the counterparts of behavioral conditioning: general activation, alerting or focusing on the relevant factors in the situation, and, finally, the specific response pattern. As the authors point out, however, the parallel is not complete because both the UR and CR are present initially to the UCS, extinguish even with repeated reinforcements, and wax and wane quite differently from an overt CR. However, the specific frequency repetitive discharge appears to be unique to the CS-US pairing and may indeed represent, as the authors suggest, a neural correlate of sensory-sensory conditioning. This is supported somewhat by the finding of Morrell, Roberts & Jasper (146) that such conditioning can be impaired by epileptogenic foci in the pertinent, but not in other, sensory receiving areas.

In an attempt to establish whether or not subcortical structures participate in the conditioning process Yoshii, Pruvot & Gastaut (239) examined the electrical activity of the brain-stem reticular formation in cats during the process of "cortical conditioning." They find in the reticular formation counterparts of the different phases of the cortical response. From their results it appears that the desynchronized phase in the development of the CCR which some investigators have related to alerting and the hypersynchronized phase which has been related to attention are similar to the two different states of excitability of the reticular formation, activation and inhibition, described by Moruzzi & Magoun (149). The temporal relationship between the CCR and activity in the reticular formation, which is so neatly revealed in the study by Yoshii, Pruvot & Gastaut, suggests that there is a reciprocal interaction between the reticular formation and the cortex during the establishment of CCRs. Whether it is to be concluded that the CCR is the correlate of an overt CR, or that it is only the effect of orienting and alerting, is still to be established. In all likelihood, as suggested by Morrell, Naquet & Gastaut (147), there is, during the establishment of the CCR, a shifting back and forth between the mesencephalic and diencephalic

parts of the activating system, resulting in the generalized and localized forms of blocking, respectively.

From their analysis of the CCR in the human, Gastaut *et al.* (65) accept Jasper's earlier interpretation of this reciprocal relationship between both "arousal systems." These authors, with Jasper, suggest that a new signal causes an alerting reaction, while repetition of the signal acts upon the thalamic component producing the progressive inhibition. They further implicate subcortical structures by pointing out that the final effect of conditioning is in the area of the cortex specific to the UCS rather than the CS. Presumably, they argue, the temporary connection established by conditioning must take place at some neural level, such as the brain stem, where both the UCS and CS impinge on common structures.

Basic to the discussion of the significance of the CCR is the validity of two assumptions. The first assumption is that the surface electrical response to the conditioning stimuli is direct evidence of the neurophysiological process of conditioning. Ricci, Doane & Jasper (185) have recently questioned the validity of this assumption. Using microelectrodes to sample the discharge of cortical cells during conditioning, they show that the pattern of surface waves in the electrocorticogram does not provide a reliable index of inhibitory or excitatory states in single units in the cortex. They point out that cortical activation is associated with a complex reorganization of discharge of cortical cells, in which inhibition is as important as excitation in the complex alteration of the patterns of neuronal discharge which constitute integrated cortical function. Their evidence suggests that conditioning involves the extinction of many irrelevant neural patterns with the emergence of significance for a given pattern. This process they claim cannot be located in restricted neuronal circuits between specific sensory and motor systems within the brain, nor can it be described in terms of local excitatory or inhibitory states.

A widely held working assumption that blocking is a sign of cortical excitation in single units, while the rhythmic slow waves represent either a lack of excitation or increased inhibition, will have to be reconsidered in the light of these recent findings. However, as Lindsley (114) has pointed out, behavior is probably a function of cell aggregates rather than individual units, and therefore relatively molar phenomena such as the CCR still warrant close analysis in the attempts to elucidate the neurological correlates of conditioning. The electrocorticogram, although not corresponding to unit firing, is probably revealing something about the organization of activity within the relevant sensory area, and, at the very least, demonstrates the plasticity in neural activity which is necessary for learning.

The second underlying assumption is that the presence of a conditioned electrical response in the brain will determine the behavior that will ensue. Chow, Dement & John (36) recently questioned this assumption. They first established in cats a CAR to a low frequency flickering light while recording the cortical responses. Finally both the CAR and the CCR were estab-

lished. Then, with the animal restrained outside the avoidance apparatus, they paired the flicker with a tone until the identical cortical response could be elicited by the tone alone. The animal was then returned to the avoidance apparatus and tested in the presence of the tone alone. Even though a localized repetitive discharge was apparent in the electrocorticogram, the animal failed to avoid the shock. It is evident that the neural mechanism reflected in the electrocorticogram is not identical to that governing behavioral learning. Until the relationships are made clear, speculation concerning the relationship between the electrocorticogram and behavior should at the least proceed cautiously.

A change in neural processes during learning somewhat different from the CCR is seen in the studies of Galambos, Sheatz & Vernier (63) and Hernández-Péon, Scherrer & Jouvét (80). They find that the amplitude and distribution of the auditory evoked potentials change when the auditory stimulus is paired with unavoidable shock. These electrical changes, apparently related to the conditioning procedure, can be observed in parts of the brain remote from the auditory pathway, for example, in portions of the hippocampus. More recently, just as has been described above for the CCR, many other subcortical structures have been shown to participate in this response; and, what may prove to be even more important, this participation is in an orderly temporal progression. John & Killam (94) have found similar effects during the conditioning of an avoidance response to a regularly flashing light. With electrodes implanted in many cerebral structures of cats, they are able to follow the growth of this conditioned electrical response. It is apparent that the response may be evident in a structure at one point in the process of learning, and not earlier or later. Perhaps this is further evidence for Jasper's notion that there is a progressive elimination of activity in irrelevant systems during learning.

The neural substrate of conditioned electrical responses.—The results of several neurophysiological studies are relevant to a consideration of the neural substrate for these electroencephalographic effects. Among these are the studies analyzing the phenomenon of habituation. Sharpless & Jasper (202) have shown that habituation does not depend on changes in the primary sensory pathways nor in the sensory cortex, but rather on changes in the diencephalic and mesencephalic activating systems. Both Sharpless & Jasper and Hernández-Péon, Jouvét & Sherrer (81) argue that afferent neural habituation cannot be explained in terms of neural adaptation, neural fatigue, or synaptic refractoriness. Thus, adaptation occurs only during prolonged continuous stimulation and subsides very abruptly after cessation of the stimulus, while acoustic habituation does not require continuous stimulation and persists for several hours after the cessation of the stimulus. Similarly, fatigue, which is generally considered to be the result of previous activity in the organ and which leads to decreased function with increased intensity, cannot account for habituation since increases in intensity actually cause dishabituation. Finally, the long-lasting nature of habituation is well

beyond the limits of refractoriness to be expected from such brief, intermittent stimuli. Habituation, and, correspondingly, dishabituation, of the conditioned electrical response as seen both in the alpha blocking and the evoked cortical potentials are therefore probably related to a system possessing the temporal characteristics necessary for learning.

There appears to be a reciprocal interplay between several levels of function which would require an effect of excitation at one level facilitating or inhibiting function at another. Galambos (62) has shown that the neural inflow of the auditory system can be suppressed by impulses, aroused in the brain, which pass out to the periphery. Rasmussen (183) has described an anatomical organization in the auditory system which could provide for this function. Hagbarth & Kerr (70) and Kerr & Hagbarth (95) have demonstrated a similar effect of cerebral stimulation on other afferent systems. Thus it appears established that considerable influence can be exerted by efferent mechanisms on afferent activity. An analogous interaction between cortical areas and brain stem has been shown by French, Hernández-Péon & Livingston (55), and Hernández-Péon & Hagbarth (79). More recently Adey, Segundo & Livingston (1) have shown that the cerebral cortex is capable of exerting a profound influence simultaneously upon sensory transmission and on transmission within the brain stem. These authors indicate that the organization of these influences appears to be such as to provide for a conjugation or integration of effects from several cortical fields simultaneously, and at the same time to provide for maintaining the identity of the cortical origin of the stimulation upon its arrival in the brain stem.

Cerebral electrical activity and complex behavior.—Several studies indicate that changes in activity in the reticular formation or its cortical regulating areas may be relevant to the organization of complex behavior. Burns (28) has demonstrated periods of activity resembling sleep following periods of stimulation of occipital cortex. These results are interpreted either as an inhibitory effect of occipital cortical stimulation on the reticular activating system, or as fatigue of the cortical cells thus depriving the reticular activating system of cortical facilitation. Neither interpretation of these findings obtained in rats is consistent with the absence of effects on the reticular formation during stimulation of the occipital region in monkeys as reported by Adey, Segundo & Livingston (1). Nevertheless, the behavioral consequences of interfering with the normal function of the cortex demonstrated by Burns emphasizes the function of centrifugal effects in maintaining the waking state.

A regulating effect on locomotor activity is also reported by Isaac & DeVito (92). They have shown in monkeys with frontal lesions a remarkable increase in motor activity in response to variations in illumination and sound. They suggest that the effects may be due to the removal of the regulating influences of the prefrontal cortex on the reticular activating system. Support for this interpretation is provided by Adey, Segundo & Livingston (1) who have shown that activity in the frontal cortex in the

region of the arcuate sulcus has a blocking effect on activity in the reticular formation. Removal of the frontal cortex would presumably free the activating system from this blocking and result in abnormal responses to stimulation. Burns (28), however, who found positive effects from occipital stimulation in rats, found no effect on behavior of stimulating frontal cortex. Thus, the generality of these findings is still in doubt.

Morrell, Roberts & Jasper (146) have shown that abnormal cortical activity may impair conditioning in monkeys. Epileptogenic lesions in the sensory receiving areas involved in the conditioning process impeded the development of the CR. These areas appear to be similar to those designated by Adey, Segundo & Livingston (1) to be the cortical areas regulating brain stem transmission. Of particular significance is the parallel report from these two studies with respect to the amygdaloid-hippocampal region. Morrell, Roberts & Jasper (146) observed that seizure activity in these structures was most disruptive of the conditioning process; Adey, Segundo & Livingston (1) demonstrated that activity in these structures resulted in the most profound and long lasting effects in the reticular formation. Perhaps, then, the impairment in conditioning in these animals takes place as much at the level of the reticular formation as at the cortical level, and would be expected from the reciprocal relationship between these levels during conditioning which Yoshii, Pruvot & Gastaut (239) have described.

A more direct demonstration of the effect of reticular-formation activity on complex behavior is provided by Fuster (61). In this study monkeys were first trained to discriminate objects, then tested for levels of performance at different tachistoscopic exposure durations. After performance levels had been well established, the reticular formation at the level of the mesencephalon was stimulated. This resulted in higher percentages of correct responses, shorter reaction times, and lowered tachistoscopic thresholds. This is a significant demonstration of the effect which activity in the reticular formation may have on perception and attention.

In humans, too, cerebral activity has been shown to be directly related to performance on complex tasks. Kooi & Hovey (102) observed the EEG of patients with subclinical paroxysmal activity during performance on Wechsler picture-completion and digit-symbol subtests. Although the hypothesis that "no-response errors" would occur concomitantly with bursts of paroxysmal activity was not confirmed by the results, it was established that more errors occurred during periods of burst activity. Mirsky *et al.* (133) compared patients with focal and nonfocal epilepsy on a repetitive visual discrimination task in which the subject is required to perform for extended periods by pressing a response key to certain letters among a series of letters appearing briefly. Only the group with nonfocal epilepsy performed significantly worse than normal. These results are taken to indicate a disturbance in the capacity to maintain attention, a disturbance which is possibly related to dysfunction in the centrencephalic system of Penfield and Jasper. This interpretation was first suggested by an earlier finding by

Primac, Mirsky & Rosvold (182) that impairment in performance on this task follows administration of chlorpromazine, a drug which is known to depress the reticular activating system. In a study in which he measured reaction time relative to the alpha cycle, Lansing (103) also demonstrates a relationship between performance and cerebral activity. He reports that shorter mean reaction times are coincident with the excitability phase of the alpha cycle.

Neural components of the CR.—With the resurgence of interest in conditioning procedures as a method of studying brain function, it is important that the newer techniques are being applied to a reassessment of the classical studies delineating the neural components of conditioning. Doty, Rutledge & Larsen (50) have validated the earlier findings of Loucks demonstrating that conditioned reflexes can be established to electrical stimulation of the cat cerebral cortex. Beck & Doty (13), by using methods of combined catalepsy and de-efferentation, have demonstrated conclusively that the overt response is not required for the formation of the CR. These studies are notable in handling efficiently many of the technical questions raised in the earlier studies and thus specify more convincingly the components necessary for establishing a CR.

SENSORY DISCRIMINATION

Auditory.—Neff and his collaborators have continued their analysis of the cortical areas serving audition by extending their lesions and investigating new auditory problems. Butler, Diamond & Neff (30) report that, after bilateral ablation of Woolsey's auditory areas I, II, and Ep, ability to discriminate changes in frequency is essentially unimpaired. Degeneration studies of the brains of these animals reveal that even though all cortex defined as auditory by electrophysiological means is ablated, the posterior tip of the medial geniculate body is spared. Only damage to that part of the cortex ventral to that defined as auditory results in degeneration to this posterior tip of the medial geniculate. Thus the authors draw the significant conclusion that if auditory cortex is defined as those regions usually delimited by evoked potential techniques, then the auditory cortex is not essential for frequency discrimination. If, however, auditory cortex is defined as those regions which receive projections from the medial geniculate body, then no study which has been reported can give a conclusive answer since complete bilateral ablation of auditory cortex has never been accomplished.

Their more recent work attempts to clarify this situation by enlarging the lesions until by any criterion they would be described as complete, and by devising tests involving different aspects of auditory behavior. Neff *et al.* (157) report the results of one such manipulation, adding a test of localization of sound in space. Ablating the same auditory areas which seem to be unessential for the discrimination of frequency changes, they find the cat deficient in auditory localization. An important observation is that the cats sometimes started for the correct goal box, but changed course en route.

This type of error was less frequent when the animals were not required to travel so far, and more frequent when a delay was interposed between stimulus and response. The last procedure is similar to that used in testing indirect delayed response, frequently considered a test of memory. In fact, Lawicka (106) who studied the effects of frontal lobectomy in dogs in an experimental situation almost identical with that used by Neff *et al.* (157) interprets the deficient performance of the dogs in terms of memory loss. Thus, as the authors imply, the results may indicate impaired memory or attention to the auditory signal equally as well as deficiencies in auditory localization.

In another manipulation of the behavioral task, Diamond & Neff (47) find impairment in discrimination of auditory patterns, but not of frequency. Analysis of the lesions in this study again reveals quite clearly the effect of extending the removal. Subtotal lesions result in partial effects, while total lesions result in complete loss of the pattern discrimination. This analysis also clarifies the anatomical question somewhat, inasmuch as the removal was large enough to include all cortex defined as auditory by the criterion of degeneration in the geniculate. Even so there is little impairment in frequency discrimination. The authors suggest therefore that cortex ventral to the neurophysiologically defined area is involved in complex discriminations. This is confirmed by Goldberg, Diamond & Neff (66) and Neff (156) who report that lesions restricted to this ventrally situated area result in a profound deficit in discrimination of auditory patterns but not of frequency.

Impairment on pattern discrimination but not on frequency discrimination is also apparent when the conditioned response is cortical desynchronization rather than conditioned avoidance as in Neff's studies. Rowland (196), in a study using implanted electrodes in cats, finds that pairing one but not another sound with shock results in a differential EEG desynchronization on a background of EEG synchrony associated with behavioral sleep. Ablating auditory cortex does not alter this differential EEG response to frequency discriminations, but, as demonstrated by Diamond & Neff (47) for the waking animal, does impair pattern discrimination. This cortex in the cat appears to be homologous to the cortex which in monkey has been found to be related to performance on visual pattern discriminations. This raises the question as to whether this cortex in the cat may not serve visual functions. Furthermore Weiskrantz & Mishkin (231) have shown for the monkey that, although damage to this ventrally situated cortex does not result in impaired discrimination of auditory stimuli, damage to frontal cortex does. Thus it may also be asked whether or not damage to frontal cortex in cat would impair discrimination even though restricted auditory cortex damage does not.

Visual.—The duplex theory of retinal function would predict that guinea pigs and cats are blind to color. On the other hand, Granit (67) might predict that these animals do have color vision since he has found that the

retinae of guinea pigs and cats in common with those of a variety of vertebrates have three classes of elements, one of which, the modulators, codes the spectral composition of visual stimuli. A similar prediction for color vision in the cat might be made from the findings of Chang (32), Lennox (108), and Lennox & Madsen (109), in which it is indicated that impulse transmission time between the geniculate and cortex differs according to the wave length of the stimulating light. Although many behavioral studies had shown the cat to be blind to color, behavioral methods with the degree of stimulus control used in the neurophysiological studies of Granit had not been used. Meyer, Miles & Ratoosh (121) and Gunter (69) have recently approached this degree of stimulus control as closely as is reasonable to expect, but still find no evidence of color vision in the cat. Nor do Miles, Ratoosh & Meyer (125) find any evidence of color vision in the guinea pig. Unless one is to conclude with Cohn (38), that the neurophysiological data are to be questioned, it would appear that certain lower mammals may have the necessary afferent apparatus for transmitting information with respect to the spectral composition of light, but they do not have the apparatus for translating this information into behavior.

Thomas & Stewart (218) point out that Köhler's field theory of cortical integration supposes that cortical neurones involved in the representation of a figure on the cortex generate potential differences between themselves and surrounding tissue. As a result, ionic currents are presumed to flow through these tissues with a distribution which depends on the particular figure as well as the particular location in the cortex. Köhler & Held (100) provide evidence for this in their demonstration that slow potential changes are correlated with movement of a visual stimulus across the visual field. More recently, Köhler, Neff & Wegener (101) have demonstrated similar phenomena in auditory cortex. Opposing this view, however, are the findings of Lashley, Chow & Semmes (105) in which crisscrossing the visual field with gold strips to disturb the normal patterns of current distribution had no effect on visual discrimination. Similarly, Sperry, Miner & Meyers (207) could demonstrate no effect on visual perception following subpial slicing or crisscrossing the cortex with tantalum implants. Both these experiments, however, only infer that the cortical fields are disrupted. Thomas & Stewart (218) present more direct evidence that disruption of the current flow does not affect perception. By passing current in either direction through electrodes placed on the vertex and occipital protuberance and thus more certainly interfering with normal distribution of figural currents, they were unable to impair performance on several visual tests.

Riesen & Mellinger (187) have examined the ontogeny of interocular transfer. In earlier studies Riesen, Kurke & Mellinger (186) and Chow & Nissen (35) had demonstrated that when one eye received only diffuse light during early development, transfer of a pattern learned by the other eye and tested in the previously covered eye did not take place. The more recent study demonstrates that practice with each eye separately during develop-

ment is the sufficient antecedent for transfer of patterns when tested later. They interpret their results in terms both of central sensory-sensory integration and sensory-motor control, stressing the effects of early training in establishing these mechanisms.

The studies on the functions of the corpus callosum in interocular transfer of training continue to contribute to an understanding of the organization of the central visual system. Myers (150, 152) had demonstrated that the interocular transfer of pattern discrimination in cats following surgical section of the crossed fibers of the chiasma did not take place when the corpus callosum was sectioned. He has since demonstrated (151) that the posterior half of the corpus callosum is more involved in this transfer than is the anterior 75 per cent. Furthermore, Sperry, Stamm & Miner (208) have shown that in animals with complete section of the callosum and chiasma, visual learning can proceed independently in the two hemispheres and there is no saving in transferring from one eye to the other. More recently, in a study testing the retention of performance of visual discrimination problems graded in difficulty, Myers & Sperry (153) have shown that there is a mnemonic carry-over via the corpus callosum into the hemisphere not directly receiving the sensory information, but that the carry-over is not sufficient to serve more difficult discriminations. That the phenomenon demonstrated for vision may be generalized to somesthesia is suggested in a study by Stamm & Sperry (209) in which a somesthetic discrimination learned by one paw of a cat did not transfer to the other paw when the corpus callosum was sectioned.

Chow (33), Mishkin & Pribram (137) and Mishkin (134) have demonstrated that visual discrimination in monkeys is impaired following lesions in the inferior convexity of the temporal lobes. These studies also indicate that a similar degree of impairment does not follow lesions in association cortex outside the temporal lobes nor does it follow lesions in the dorso-lateral surface, the pole, amygdaloid nucleus, or the hippocampal formation within the temporal lobes. Orbach & Fantz (165), Riopelle & Churukian (189), Wilson (235), Pasik *et al.* (166), and Pribram & Barry (180) have independently confirmed that lesions restricted to inferotemporal cortex result in such visual discrimination impairment. That this impairment might be specific to vision is suggested by the findings of Pribram & Barry (180), Wilson (235) and Pasik *et al.* (166) that inferotemporal lesions do not impair tactual discrimination, and the finding of Weiskrantz & Mishkin (231) that such lesions do not impair auditory discrimination.

Against these conclusions, however, are the recent findings of Hamuy *et al.* (71) suggesting that temporal cortex outside the inferotemporal area may be most important for visual discrimination, and of Santibanez & Hamuy (197) that these same lesions, in addition to producing deficits in visual discrimination, may also produce deficits in olfactory discrimination. The lesions in these studies, however, unlike those in the others that have been referred to, include temporal polar and perimygdaloid cortex. The possibility that the olfactory discrimination impairment resulted from this

particular extension of the inferotemporal lesion deserves further investigation. Indeed, the temporal pole has already been strongly implicated in olfactory functions on the basis of anatomical and electrophysiological studies reviewed by Pribram & Kruger (176) and, according to the neurophysiological evidence of Segundo, Naquet & Arana (201), the pole is functionally quite unrelated to the inferotemporal cortex.

Whether the inferotemporal neocortex is a focal area serving visual functions only, or visual functions predominantly, its relationship to the primary visual system still requires elucidation. The possibility that the essential connection is by way of the pulvinar as might be suggested from the evidence of Jasper, Ajmone-Marsan & Stoll (93) and by Niemer & Jimenez-Castellanos (158), or by way of the superior colliculus as might be suggested by Whitlock & Nauta (233), has been tentatively ruled out by studies by Chow (34) and by Rosvold, Mishkin & Szwarcbart (195) in which lesions in these structures failed to result in visual discrimination deficits. On the other hand Ettlinger (51) and Mishkin (136) have provided support for the alternative possibility that the inferotemporal and primary visual areas are linked more directly by cortico-cortical connections.

Both these studies involved precluding input to one striate cortex by cutting the optic tract or by removing an occipital lobe, and then ablating the contralateral inferotemporal cortex. Following these operations the animals were retrained on visual discriminations and then subjected to sectioning of the corpus callosum, thereby cutting the cortical connections between the intact geniculo-striate system of one hemisphere and the intact inferotemporal cortex of the other hemisphere. Both studies find clear evidence of impairment, thus suggesting a relationship between striate and inferotemporal cortex. What this relationship is, however, has not yet been determined, but one reasonable guess, based on the strychnine neurographic evidence of von Bonin, Garol & McCulloch (17) is that the pre-striate cortex may relay activity between the striate and inferotemporal areas of the same hemisphere and between the striate areas of opposite hemispheres. If this suggestion is correct, then the failures to find discrimination loss after pre-striate lesions, as reported by Lashley (104) and by Chow (33), are likely due to incomplete removals of pre-striate cortex.

Support for the notion of an interrelationship between the inferotemporal cortex and primary visual system is provided in a recent study by Dorff (49). He has shown that patients with unilateral temporal lobe removals for relief of epilepsy have a lowered recognition for tachistoscopically presented letters in the visual half-field contralateral to the side of the removal even though the stimuli are exposed in a part of the field which is apparently free of primary visual loss. The evidence from this study supports the earlier finds of Milner (126) which implicate the temporal lobes of man in complex visual functions. Whether or not in humans such effects are restricted to removals of temporal cortex is questioned in the

studies of Battersby, Krieger & Bender (10) and Teuber & Weinstein (216) which confirm their earlier findings that in man the location of a unilateral lesion is not a major factor in producing decrements in visual functions.

Another difficulty for interpreting the visual deficit following temporal lobe damage in man in terms of an impairment in some complex visual function is raised by Pollack, Battersby & Bender (169) in a study of tachistoscopic identification of contour in patients with brain damage. They also demonstrate impairment in visual recognition in fields which appear to be normal, but these authors are more inclined to an interpretation in terms of a subtle acuity loss due to primary involvement combined with nonspecific effects of brain damage similar to those discussed by Teuber & Weinstein. At the present time, the evidence in man appears to be insufficient to choose between these two positions.

The evidence for interpreting the visual deficit in the monkey is perhaps less equivocal. Mishkin & Hall (138), in a study of size discrimination, obtained results suggesting the possibility that the discrimination loss in monkeys, also, is due to decreased visual acuity. Wilson & Mishkin (236) therefore compared monkeys with lesions in the inferotemporal region with others having partial striate lesions. They used several tests, some intended to emphasize acuity and some discrimination learning. The results showed that although both types of operates are inferior to normals on both types of tests, striates are inferior to temporals on tests of acuity, and temporals are inferior to striates on tests primarily involving discrimination learning. These results clearly favor the view that the deficit following temporal lobe damage is not to be explained simply in terms of acuity loss, but rather in terms of loss in some more complex visual function related to learning. Pribram & Mishkin (177) provide support for this interpretation in a study demonstrating discrimination impairment in inferotemporal operates to successively presented stimuli, even though the identical stimuli had been successfully discriminated earlier when they were presented simultaneously.

In all of these studies of discrimination loss following inferotemporal lesions, the degree of loss has been associated with the difficulty of the task for normal animals. Chow & Orbach (37) attempted another test of this relationship in a study in which the level of difficulty was defined in terms of the exposure duration of the visual stimuli. Temporal-lobe animals were not differentially affected in this test. Thus it is concluded that the level of difficulty of the problem is in itself not sufficient to explain the deficits which appear following temporal lobe lesions.

This study also demonstrates that the deficit cannot be attributed to impairment in attention or vigilance. That memory for the relevant factors in the discriminanda is not a factor is evident in the report of Riopelle & Churukian (189) in which temporal lobe operates were not affected by increasing the intertrial interval in discrimination problems. On the other hand, it was observed in Chow & Orbach's study that the degree of deficit did appear to be related to another parameter which could be interpreted in

terms of retention difficulty for normal animals, namely, the degree of overtraining before operation. Orbach & Fantz (165) confirmed this in a study in which they show that the degree to which a visual discrimination habit is affected by the lesion depends on the amount of preoperative overtraining on that habit. Thus, it is evident that increasing the difficulty of a visual discrimination task for normal animals only along certain dimensions determines the severity of the impairment following inferotemporal lesions.

COMPLEX BEHAVIOR

Analysis of delayed-response deficits.—The analysis of the delayed-response deficit characteristic of frontal-lobe damage in monkeys continues to receive attention. This research has taken four directions: determination of the critical cerebral focus for delayed-alternation and delayed-response deficits in monkeys, determination of the test variables which are related to the deficit, analysis of this impairment in terms of classical conditioning theory, and the determination of other functions which may be disturbed by frontal lesions, particularly in man.

Pribram *et al.* (172, 175, 178) had demonstrated that the cortical areas focally concerned in delayed-response types of functions are restricted to the lateral surface of the frontal lobes anterior to the arcuate sulcus. Mishkin (135) has recently confirmed and extended Blum's (16) earlier findings in demonstrating that equally severe deficits in this behavior may be produced by lesions in the midlateral region along the sulcus principalis as by lesions of the whole dorsolateral convexity. Such a precise localization of function should make easier the task of delineating the neural mechanism which mediates this class of problem-solving behavior. One approach to this problem has been to observe the effect on behavior of destroying subcortical structures which project to the frontal granular cortex. Pribram, Chow & Semmes (173) have analyzed the topographic organization of the most obvious of these projections, namely, that from the medialis dorsalis nucleus of the thalamus. It was reasonable to suppose that destruction of this nucleus would result in impaired performance on delayed-response tests, but that such is apparently not the case has been shown by Chow (34) and Peters, Rosvold & Mirsky (167) who have demonstrated negative effects even from massive lesions in this subcortical structure.

Another subcortical structure, however, does appear to be involved. Rosvold & Delgado (194) have shown that either electrical stimulation or destruction of tissue in the head of the caudate nucleus interferes with performance on delayed alternation. This finding has since been confirmed by Rosvold, Mishkin & Swarcbart (195) for restricted electrolytic lesions and by Migler (123) for large ablations of tissue in this nucleus. It thus becomes important to determine whether or not the head of the caudate nucleus is related to the midlateral frontal cortex through direct projections, or indirectly through some connection it has in common with some other structure.

Harman *et al.* (75), and Mettler, Hovde & Grundfest (119) have pre-

sented anatomical and electrophysiological evidence suggesting that the connections are direct. However, this has been denied in most anatomical studies such as those of Levin (110) in which no connections were observed. In addition, electrophysiological studies of Forman & Ward (54) would indicate that the out-flow from the caudate nucleus and the frontal cortex is most probably along separate anatomical paths. Furthermore, Poggio, Walker & Andy (168), and Walker, Poggio & Andy (223) have shown that, although there are preferential pathways of propagation of electrical discharges between frontal granular cortex and the caudate nucleus, the evidence favors some intervening mechanism.

Recent reports by Rosvold, Mishkin & Szwarcbart (195) that delayed-alternation deficits may also appear following lesions in rhinencephalic structures emphasize that the neural substrate of this behavior is probably very complex. Whatever the connections, it is clear from recent evidence presented by Orbach (164), in which he shows that the impaired performance is apparent within a few hours of surgery and improves very little with training or recovery, that the effects of frontal-lobe damage are a result of damage to the neural mechanism essential for delay-alternation performance and cannot be attributed to indirect effects of the primary lesion.

A great deal of evidence has accumulated with respect to the behavioral analysis of the deficit. A delay between stimulus and response is the most obvious feature which is common to tests that have revealed a greater impairment in monkeys with frontal lesions than in monkeys with lesions in the other cortical areas. Mishkin & Weiskrantz (141) have shown that a delay between response and reward may be equally as effective in revealing the deficit. However, Riopelle & Churukian (189), in demonstrating that frontal operates are successful on discrimination-type tests even with very long intertrial delays, emphasize the necessity of clarifying the role of intratrial delay in delayed-response performance. It is likely, as suggested by Mishkin & Weiskrantz, that the intratrial delay is not a sufficient condition for eliciting impairment.

Mishkin & Pribram (139, 140) and Pribram & Mishkin (179) have attempted an analysis of the nondelay factors in delay-type problems and have tentatively ruled out spatial aspects of the problem as critical in determining the deficit. Examining the performance of the frontal operates in this and several other studies, they concluded that factors in the test which increase the distinctiveness of the cue at baiting improve performance. As Stellar (210) has pointed out, however, such an interpretation may be adding nothing new to the older hypotheses of inattention or distractibility in accounting for the impaired performance. The cue distinctiveness hypothesis does, however, suggest a different set of experiments which may help to elucidate the behavioral mechanisms involved in delayed-response behavior.

Another line of analysis is suggested by the results of an experiment by Teuber & Mishkin (215) using human subjects. Teuber & Mishkin have interpreted the results of this study as showing that frontal damage pro-

duces a disturbance in the integration of postural and visual, and possibly other exteroceptive cues. This finding suggests that perhaps in monkeys the delayed-response deficit also may be related to the inability of the animal to integrate postural and exteroceptive cues. Such an account would be in accord with Mettler and associates' (120) general interpretation of caudate nucleus damage, which is that the primary dysfunction is an inability of the animals to relate themselves efficiently, in terms of postural mechanisms, to the environment. This might lead to an inability of the animal to maintain orientation during the delay period. Indeed, this is Lawicka's (106) interpretation of the delayed-response deficits following frontal lobectomy in dogs.

Pribram (171) and Pribram & Bagshaw (174) have suggested another possible explanation. They suggest that the impaired performance may be related to the decreased reward value of the food. Supporting this conclusion is the evidence that the effect of nembutal, which apparently increases the reward value of food, also improves an animal's performance on delayed-response for food rewards. However, Miles & Rosvold (124) have demonstrated that the impairment in delayed-response is the same whether the reward is food or escape from pain. It is likely, therefore, that the impairment is independent both of the particular reward being used and of the particular motivational level.

Fundamental to this research on frontal lobe function is the claim that frontal ablations result invariably and selectively in delayed-response deficits. Both points have been questioned recently. Warren *et al.* (225), citing earlier experiments, point out that in some preparations, at least, a significant capacity for solving delayed-response and double-alternation problems remains after extensive lesions of the frontal association areas. Furthermore, in studies by Harlow (72), and Riopelle & Pfeiffer (190), it is evident that, in visual learning-set problems, frontal and temporal operates are similarly impaired. In addition it is indicated in a study of Weiskrantz & Mishkin (231) that the prefrontal lobes may also be involved in auditory discrimination. Finally, the results of Konorski and his group, working with dogs, indicate that frontal-lobe damage, in this species at least, produces profound disinhibition in conditioning procedures. The frontal lobe syndrome in animals thus appears to be far more general than had been thought to be the case.

A different type of analysis is provided by Konorski and his colleagues in terms of classical conditioning concepts. Bruckowski, Konorski and others (26) have examined the effect of the removal of the frontal poles of the cerebral cortex on motor conditioned reflexes in dogs. After bilateral frontal ablations, excitatory conditioned reflexes remain unimpaired while inhibitory reflexes are disinhibited. In this behavior, however, the animals regain their original levels of response, but in degrees depending on the extent of the inhibition required in the problem. Performance on an alternation problem is never recovered, and is considered by this group of investigators to

represent the highest manifestations of impaired inhibitory processes. Lawicka (107) reports similar results for vocal conditioned reflexes and makes the point that this response, unlike limb movements, is unaffected by changes in the locomotor activity in the animals, a criticism that had been raised against the earlier study. Brutkowski (27) reports similar effects of frontal lobectomies on salivary conditioned reflexes. Lawicka (106) has also demonstrated that frontal dogs could not perform delayed response even when the cues remained visible during the delay if the dogs did not maintain their orientation towards the stimuli. Perhaps the greatest significance of these findings of Konorski and his colleagues is that they provide a methodology in which relevant variables can be manipulated more directly than has been possible in behavior as complex as that in the delayed-response problem.

Brain damage and general intelligence.—Recent studies with respect to the effect of brain damage on general intelligence, though more rigorous than in the past, are no more in agreement than were earlier studies, some of which claimed deterioration in intelligence, others not. Equally lacking in agreement are the conclusions as to whether these effects may be localized and whether they are specific to one or another modality. Morrow & Mark (148) compared the performance on the Wechsler Intelligence Scale of male subjects with brain pathology, in which autopsy material confirmed the site of the lesion, with matched psychiatric controls. They conclude that intelligence of the brain-damaged is significantly decreased. Furthermore they conclude that performance scores are more impaired than verbal scores, that greater deficits appear in subjects with lesions in the posterior than in the frontal areas, and that greater deficits are apparent in subjects with lesions in the dominant than in the nondominant hemisphere.

Ideally, such comparisons should be made on the basis of pre-and post-injury scores in matched groups. Although it is difficult to obtain such measures, it is possible to do so in veterans, most of whom are given a general intelligence test upon induction into the service. Several investigators have made use of such material. Rosvold & Mishkin (192) and Hoyt, Elliott & Hebb (87) compared intelligence test scores which had been obtained on Canadian veterans at induction with scores on the same tests obtained on the same subjects some years later after intervening psychosis and lobotomy. From these comparisons they conclude that there is a loss in general intelligence attributable to frontal lobotomy. Ross (191) demonstrated a loss in intelligence from a comparison of classification test scores obtained on American Army veterans at induction and later after brain injury without intervening psychosis. He did not, however, use the same test in making the comparison, but depended on correlated measures.

In a far more extensive study, Weinstein & Teuber (228) achieved the ultimate with this technique by comparing scores on the Army General Classification Test obtained on subjects 10 years after injury with scores which had been obtained on the same subjects at induction. This comparison involved scores on 62 men who subsequently sustained peripheral nerve

injury. Their data indicate that the intelligence test scores are significantly lower than the preinjury level only in groups with injury in parietal and temporal lobes of the left hemisphere. Their data also show, however, that all groups, irrespective of lesions, fail to increase as much as the controls, a finding which in an experimental design such as that used by Morrow & Mark (148) would be interpreted as a generalized impairment in intelligence. A general conclusion as to whether specific or general effects follow localized brain injury is probably premature for, as Teuber & Weinstein (217) point out, the effect which is obtained depends to a considerable extent on the range of tasks employed in the testing procedure.

Another question which has been raised concerning the effects of brain injury in humans is whether or not the level of preinjury intelligence or education may determine the extent of impairment following injury. Weinstein & Teuber (227) report that there is no significant differential effect of brain injury, depending either on different levels of intelligence or of education. This is in contrast to earlier results reported by the same authors demonstrating less effect of injury on specific tasks on which brain-injured have more experience than controls, and emphasizes that following brain injury specific experience is probably less vulnerable than general intelligence. Similar conclusions are apparent in results reported for monkeys in learning-set problems by Harlow (72) and by Orbach & Fantz (165), which indicate that increased amounts of preoperative training may lessen the effects of brain injury.

There is more, but not complete, agreement with respect to the fact that lesions in the dominant hemisphere, particularly of the parietal and temporal regions, result in more severe loss than lesions in the nondominant hemisphere. Milner (126) has shown this to be the case with lesions restricted to the left temporal lobe, and Weinstein & Teuber (228) with lesions in the left parietal lobe. Morrow & Mark (148) also find this to be true for the Wechsler scores, in both verbal and performance IQ. Reitan (184), however, finds the deficit in the dominant hemisphere to be only in the verbal subtests, the performance subtests scores being affected by lesions in the nondominant hemisphere. Heilbrun (78) presents evidence which confirms the finding of greater deficits in verbal tests following lesions in the left hemisphere but no differences between the two lobes in tests involving only performance tasks. Battersby *et al.* (11) also find both hemispheres to contribute equally to deficits on a task involving identification of spatial aspects of complex stimuli.

Several reports have described the effect of temporal lobe lesions on complex behavior. Battersby *et al.* (11), on the basis of an extensive study on responses of brain-injured patients on tests designed to reveal asymmetric responses to stimulation of the left and right halves of the body, conclude that lesions of the parieto-occipital and temporo-occipital areas of either hemisphere most frequently result in asymmetrical perception. However, they interpret their results as indicating that location of lesion played

only an indirect role, the primary defect being attributable to defective sensory input.

Milner & Penfield (127) had reported earlier that recent memory loss is evident only after bilateral temporal lobe involvement. Scoville & Milner (200) obtained support for this finding in a study involving cases in which the degree of memory loss was correlated with the amount of hippocampus removed bilaterally. Walker (224) questioned the necessity of bilaterality with evidence he had from four cases in which there was evidence that only one lobe was involved.

Milner (126), in a more recent comprehensive report based on extensive psychological testing before and after unilateral partial temporal lobectomy in over 100 cases of temporal lobe epilepsy, summarizes the state of affairs as follows: intelligence as measured by the Wechsler-Bellevue Intelligence Scale is not permanently affected, although there is a deficit on verbal subtests in the left-temporal group during the period of post-operative dysphasia. Longstanding epileptogenic lesions of the temporal lobe are associated with effects on certain specialized tests, these defects varying in kind and depending on whether the lesion is in the dominant or the nondominant hemisphere. Unilateral and epileptogenic lesions of the dominant (left) temporal lobe are accompanied by difficulties in verbal recall, although recall of nonverbal material is normal. Unilateral epileptogenic lesions in the nondominant (right) temporal lobes are accompanied by impairment in the comparison of pictures, although verbal skills are intact. When unilateral partial temporal lobectomy is carried out for the relief of seizures, these specific deficits persist, and in fact tend to be accentuated. This is true even in those cases which show a postoperative increase in IQ rating and complete cessation of seizures. In contrast to the relatively mild deficits which accompany unilateral lesions, bilateral damage to the hippocampal zone causes profound and generalized loss of recent memory, unaccompanied by other intellectual changes. Milner concludes from these findings that (a) the left temporal lobe contributes to the understanding and retention of verbally expressed ideas, (b) the right temporal lobe aids in rapid visual identification and (c) the hippocampus and hippocampal gyrus (either separately or together) play a crucial role in retention of new experience. Meyer & Yates (122) arrive at substantially the same conclusions in a review summarizing Meyer's work with Falconer investigating the effects of left temporal lobectomy.

EMOTION AND MOTIVATION

Sexual and maternal behavior.—Beach, Zitrin & Jaynes (12) review the literature with respect to the effects of cortical lesions on sexual behavior of rats, rabbits and cats which, together with new experimental evidence of their own, indicates that in the rat and the cat, but not in the rabbit, bilateral removal of neocortex in the male abolishes all sexual behavior. Sexual behavior is greatly increased, however, as originally reported by Klüver

& Bucy (99) for the monkey and by Schreiner & Kling (198) for the cat, by bilateral ablation of the pyriform cortex and underlying amygdaloid nuclei. Morgane & Kosman (144), however, report no such effect. Green, Clemente & de Groot (68), in an extensive study to determine the locus of these effects, confirm the earlier findings in cats and clearly show that lesions of the pyriform cortex alone will produce hypersexuality, thus suggesting that neither the amygdala nor hippocampus is necessarily involved. On the other hand, MacLean (116) has demonstrated increased sexual activity in male cats following electrical or chemical stimulation of the hippocampus, and Fisher (53) increased sexual behavior in rats from chemical stimulation of the lateral preoptic area of the hypothalamus. There appears to be a species difference in the function of the pyriform-amygdaloid structures in sexual behavior, however, since, contrary to the reports for cats, Fuller, Rosvold & Pribram (58) find no effect on sexuality following such lesions in dogs.

In summary, then, it appears that conclusions with respect to the effects of brain lesions on sexual behavior may not be generalized from one sex to another nor from one species to another, that it is not yet certain for any one species which specific structures are critical in producing the effects, that sexual behavior probably has a wide neural representation with a complex interaction between old and new brain structures and between neural and humoral agents rather than restricted locus in a sex center in the cerebrum.

Feeding and drinking behavior.—Andersson & Wyrwicka (6) review the studies indicating that the hypothalamus contains an area focally concerned with drinking behavior and present new data indicating that electrical stimulation of this area, and no other in adjacent diencephalon, will, in thirst-sated goats, elicit an instrumental conditioned response to obtain water. These authors interpret their results as indicating that the hypothalamic center is the origin of the unconditioned reflex for drinking which is connected with other centers during the establishment of the conditioned motor reaction.

Increased food intake is described following lesions in the amygdaloid complex for cats by Morgane & Kosman (144) and by Green, Clemente & de Groot (68), and for dogs by Fuller, Rosvold & Pribram (58). This phenomenon had been observed earlier in rats in many studies reviewed by Teitelbaum (213) following destruction of the ventromedial nucleus of the hypothalamus. Teitelbaum (213) has continued his earlier analysis (212) of the drive changes in hypothalamic hyperphagia and extends his findings and those of Miller, Bailey & Stevenson (131) to show that both for random and food-directed activity, as well as for other measures, hyperphagic animals, especially obese animals, have a lower than normal drive for food.

Aversive behavior.—Many recent reports confirm the early suggestions that the medial and basal cortex of the cerebral hemispheres serves as a

neural substrate of affect or emotion. Continuing with the methods originally developed by Hess (82), Hunsperger (90, 91) has confirmed that integrated rage reactions culminating in attack may be elicited by electrically stimulating not only the preoptic region of the hypothalamus but also the midportion of the midbrain central gray. Lilly (111) describes similar emotional reactions upon stimulating the hypothalamus of the unanesthetized monkey. Shealy & Peele (203) and Molina & Hunsperger (142) have further demonstrated that this affective reaction, differing only in the frequency of attack, may be elicited by stimulating the amygdala. The latter authors also clearly demonstrate that these effects are obtained only from the nucleus basalis, nucleus centralis, and nucleus medialis of the amygdala, regions from which several components of the stria terminalis arise, connecting the amygdala to the brain stem.

Fear-like behavior has been elicited from monkeys by Delgado (43) and MacLean & Delgado (117) with electrical stimulation of the amygdala, and MacLean (116) and MacLean *et al.* (118) using both chemical and electrical stimulation have related the hippocampus alone to fear-like behavior. Delgado, Rosvold & Looney (46) have demonstrated that a conditioned-fear response can be evoked by electrical stimulation of amygdala and of central gray, and Delgado, Roberts & Miller (45) and Bursten & Delgado (29) have demonstrated that stimulation of amygdaloid structures leads to fear-like responses which have all the characteristics of learned behavior. However, the many anomalous relationships between the evoked emotional response and conditions of reward and punishment recently described by Miller (128) suggest that it is premature to conclude that the centrally evoked response is identical with the exteroceptively evoked response.

The earlier evidence suggesting that the amygdala, hippocampus, and hypothalamus were part of an anatomical system serving emotional behavior has found confirmation in the more recent electrophysiological evidence of Adey, Sunderland & Dunlop (3) and the anatomical findings of Nauta & Valenstein (155) and Adey & Meyer (2). That periaqueductal central gray may also be considered part of such a system is suggested by Nauta (154).

The findings in animals suggesting that the anteromedial portions of the temporal lobe are concerned with emotional reactivity have, in general, been confirmed in humans. Terzian & Ore (214) report a case of bilateral removal of the temporal lobes in man resulting in the gross behavioral changes first described in the monkey by Klüver & Bucy (98, 99). The most dramatic of these changes are apparently the loss of fear and rage reactions, and increased manifestations of sexuality. Similar changes in affective and sexual responses in humans from electrical stimulation of temporal lobe structures are described by Monroe *et al.* (143) and by Higgins, Delgado & Hamlin (83).

Several recent studies have been undertaken with rats, cats, and monkeys to elucidate the function of the rhinencephalic structures in acquired fear, and to demonstrate the critical focus for these functions. Brady *et al.*

(24) report that rhinencephalic injury in the cat largely restricted to the amygdaloid nuclei can affect differentially the acquisition and retention of a conditioned avoidance response. Although acquisition of the CAR is much slower in the amygdalectomized animals, retention is not impaired. On the other hand, animals with orbitofrontal lesions sustain complete retention loss and fail to reacquire the conditioned avoidance response. These authors interpret their results to indicate that the amygdala plays a facilitative role in the acquisition of a CAR but does not function in its maintenance.

That maintenance of a CAR may be a function of the prefrontal lobes is suggested by Waterhouse (226) in a study in which he finds a complete loss of a conditioned fear response, impaired reacquisition, and more rapid extinction of the reacquired response.

Weiskrantz (229) reports that not only after lesions in the frontal lobe but also after lesions in the amygdala, there is more rapid extinction of a CAR and also of a conditioned suppression response, but only if this had been established preoperatively. He confirms in monkeys the earlier findings of Brady *et al.* (24) that in cats not only the acquisition of the response but also the maintenance of the response is impaired. This author suggests that the facilitative role which the amygdala might play in the acquisition of a response is in making it possible for reinforcing stimuli to become established as such.

Pribram & Weiskrantz (181) have attempted to specify which part of medial and basal cortex is specifically related to the CAR. In this study, monkeys with lesions not only in the allojuxtallocortex comprising frontotemporal, medial frontal, frontal-cingulate, and Ammon's formation, but also animals with lesions in the anterofrontal isocortex, showed immediate extinction and slower reacquisition of a preoperatively acquired CAR. Occipitoparietal and inferotemporal operates did not show rapid extinction. However, since the frontal animals did not show a rapid rate of extinction of the reacquired response while allojuxtallocortex animals did, these authors attribute the effects in the frontal animals to an artefact of their hyperactivity, and thus conclude that the phenomenon is specific to lesions of allojuxtallocortex. It should be noted, however, that hyperactivity would not explain the impairment in conditioned avoidance in Waterhouse's monkeys with frontal lesions; nor would it explain the rapid extinction and impaired reacquisition of an avoidance response reported in a study by Thompson (221) in cats which are less active after caudate lesions. It is probable therefore that the effects of frontal lobe damage are to be accounted for in terms other than hyperactivity.

In an attempt to explain the changes in the CAR, Weiskrantz & Wilson (230) have examined the effect of lesions in rhinencephalic structures on avoidance thresholds. The animals with extensive lesions had a raised threshold, then a threshold even below the preoperative level. They claim these results suggest that the impairment in the CAR demonstrated earlier is probably due to a relearning deficit in which it is assumed that the basic

deficit in these animals is a difficulty in the formation and maintenance of the association between primary and secondary reinforcing stimuli.

Thomas & Otis (219) offer a similar interpretation of results in a study with rats in which deep lesions were placed in the rostral tips of the hippocampus. Inasmuch as these animals did not show any decrement in the conditioned fear response, but only in the conditioned avoidance response, the authors suggest that such lesions probably cause deterioration in performance of any complex learning task, whether emotional or not. In a later study, Thomas & Otis (220) showed this to be the case, since animals with lesions in the rhinencephalic system were deficient in learning a maze problem. These results are interpreted as indicating interference with the effects of general reinforcement rather than with emotional behavior *per se*.

Also emphasizing a more general rather than emotion-specific function for the rhinencephalon is Correll's (40) study. Correll, working with cats, found that, while stimulation of the hippocampus does not affect a conditioned emotional response, it does affect the rate of extinction of this response. All this evidence taken together, then, suggests that the rhinencephalic system is involved in more than the elaboration of emotional behavior, that other structures may also be involved in this behavior, and that interpretations exclusively in terms of changes in emotional behavior are unwarranted, many of the changes perhaps being more related to processes involved in learning.

Somewhat related to these studies are those dealing with the relationship between the hippocampal system and aggressive behavior. It would be expected that the social behavior of animals, so dependent upon aggressive submissive interactions, would be altered following lesions in the rhinencephalic structures. This was found to be the case for monkeys in a study by Rosvold, Mirsky & Pribram (193) in which dominant animals in a hierarchy fall in dominance following lesions in the amygdala. This finding has been confirmed in dogs by Fuller, Rosvold & Pribram (58), and Alonso-de Florida & Delgado (4) have shown that prolonged stimulation of amygdala in cats results in changes in aggressive behavior in social situations. That the limbic system is not equipotential in this respect however is suggested by Mirsky, Rosvold & Pribram (132) who found that cingulectomy alone does not result in changes in interanimal behavior, even though many earlier studies which they review claim that it does.

Brady & Nauta (22, 23) demonstrated that lesions in the septal fore-brain of the rat increase the emotional reactivity of these animals and, at the same time, decrease the strength of the conditioned emotional response for about 60 days after surgery. They conclude that the decrease in the CER may have reflected an impaired discriminative function rather than decreased emotional reactivity. Tracy & Harrison (222) have recently found, however, that a conditioned aversive response is abolished while a discriminative response to the aversive stimulus is maintained. This evidence would suggest that the decrease in the strength of the emotional response

is not a function of impairment of discriminative functions, but does indeed represent a change in emotional reactivity. Brady & Nauta had suggested that lesions in the columns of the fornix were most effective in producing these effects. Harrison & Lyon (76), however, in an extensive study in an attempt to determine the locus of these effects, conclude that there is no consistent relation between postoperative behavioral changes resembling fear or rage and either small lesions restricted to parts of the septal nuclei or extensive lesions including all of the septal region and the columns of the fornix. Their evidence suggests that when behavioral changes are obtained following septal lesions, they are due to incidental damage to other structures. They were unable to identify these other structures.

Rewarding effects of brain stimulation.—Limitations of space do not permit adequate review here of the many recent studies dealing with the relationships between brain function and pleasurable behavior. These studies, utilizing techniques of electrical and chemical stimulation, including self-stimulation, have been adequately reviewed by Zeigler (240), Brady (20, 21), and Miller (128, 129, 130) and will certainly be completely covered by Olds in his forthcoming review for the *Annual Review of Physiology*, Volume 21 (1959). Suffice it to say here that the recent studies of Olds (160), Sidman *et al.* (206), and Brady *et al.* (25) add to the original findings of Olds & Milner (161) and Olds (159) the crucial information that self-stimulation has the properties characteristic of primary rewards. It is thus reasonable to conclude with the authors that the brain structures responsive to this stimulation are indeed part of the neural substrate through which positive reinforcements influence behavior.

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FURTHER DEVELOPMENTS IN PSYCHOLOGY IN THE U.S.S.R.

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This paper is a sequel to a review of psychology in the U.S.S.R. which appeared in last year's *Annual Review of Psychology* (1). It is mostly based on material which became available in this country (as indicated by the *Monthly List of Russian Accessions to the Library of Congress*) after the completion of the bibliographic work for the earlier review in the spring of 1957. Not all of this material is actually new. Some of it was available in the U.S.S.R. two or three years ago, and in one case the proceedings of a scientific meeting which took place in 1953 did not appear until 1957. This review also includes some material which was listed as available a year ago, but which could not be obtained then or was not utilized for other reasons. Some of the material included consists of new editions of books which had appeared earlier. Thus this review is not intended exclusively to present the developments in psychology which took place last year in the U.S.S.R. but also to attempt to present additions to the picture of Soviet psychology previously presented.

A new textbook.—It was pointed out in the earlier review that the status of psychology appears to be improving in the Soviet Union. Additional facts strengthen this impression. One of these facts is the appearance of a new psychology textbook (2) edited by the president (Smirnov) and three vice-presidents (Leontiev, Rubinstein and Teplov) of the new Society of Psychologists. The textbook, which is intended for teachers' colleges, is far superior to the earlier ones which were reviewed a year ago. The book is much closer to the factual level of psychology than the earlier ones. No irrelevant (or any other) references to Stalin were found. There is no glowing description of the "typical new Soviet man", although a picture somewhat similar to him is presented as a desirable educational aim and as something to hope for. There seem to be no blanket indictments of foreign psychology; instead, there are only a few references to "some reactionary" foreign psychologists. There is no evidence of avoidance of credit given to foreign scientists.

On the positive side, there is much more documentation of the various points made by the authors; there are many descriptions of supporting experiments and observations; and there are many tables and graphs. The names of the authors who reported the experiments and observations are generally given, although usually without additional bibliographic information. More topics are presented in the new book than in predecessors; there are 20 chapters instead of 12 to 15. The "newest" chapters are those dealing with "higher nervous activity" (Chap. 2), with "types of higher nervous

activity and temperament" (Chap. 16), with practiced skills (Chap. 14), with motivation (Chap. 11), with speech (Chap. 9), and with human development from infancy to maturity (Chaps. 18, 19, 20).

The chapter with the Pavlovian title, "Higher Nervous Activity" (by Luria, assisted by Poliakov) is an informative and fairly detailed presentation from the Pavlovian standpoint of the anatomy and physiology of the nervous system. Discussions of the effects of brain injuries, e.g., of aphasia and agnosia, are included. There are some differences between interpretation of physiological facts as presented in the book and as viewed in this country. For instance there seems to be no reference to the all-or-none law, which is not accepted in Russian physiology. On the other hand, a considerable amount of attention is devoted to "phasal" (fazovye) phenomena, first described by Vvedensky (3), in which the normal intensity relations between stimuli and responses are disturbed and which serve the purpose of protecting the nerve cells from exhaustion and damage. Ukhtomsky's theory of the dominant (3) also is presented.

The chapter on "types of higher nervous activity . . ." (by Leites), presents much the same material on Pavlovian typology (as modified by Teplov) as has been presented in earlier books and periodical literature, but in considerably greater detail. An example of the greater detail is the presentation of more facts about the two boys who are frequently used as evidence in the Russian psychological literature in arguments for Teplov's view (According to this view all types of nervous functioning are potentially equally valuable to society but exhibit qualitative differences). Both boys were "gold medal" winners for excellence in secondary school work. One is described as representing the "strong mobile type," the other as having a weak type of nervous functioning. The several earlier accounts seen by this reviewer did not indicate in what way the two boys were qualitatively different and what constituted the compensating features in the case of the "weak-type" boy. In the chapter under discussion the material is presented. The first one of the boys is described as able to accomplish a great deal of work, as requiring little rest, and as grasping new material very readily. The second boy is said to be far inferior to the first one in these respects but, not being able to grasp new material quickly, he tends to go over the material repeatedly and thoughtfully and often acquires deeper understanding of the material than the other boy, often doing qualitatively better work.

The chapter on practiced skills (by Gurianov) contains much quantitative information on the effects of practice on skill; it also contains what impresses this reviewer as a reasonable solution to the controversy over the relation between practiced skill and "know-how", which was mentioned in last year's review. The author states that a certain amount of "know-how" may develop without practice, as a result of observation or verbal explanation, but is usually only perfected on the basis of developing practiced skills.

The chapter on motivation ("Needs and Motives of Activity," by

Leontiev) is one of the shortest in the book (There were no corresponding chapters in the books examined earlier). Needs are characterized as resulting from conditions of lack in the organism and as leading to increased sensitivity of the organism to certain influences which are capable of alleviating the lack. In more complex cases, the organism becomes sensitive to aspects of the environment which do not gratify the need, but which act as signals of other aspects which do; thus the smell of food becomes connected with its other aspects and acquires the power to evoke salivation. Such conditioned sensitivity to signals may turn into unconditioned sensitivity in the course of evolution.

There is no enumeration of human physiological needs. The author does point out that man has a particularly complex set of needs which acquire "qualitatively new peculiarities" and which function in accordance with new laws, these new laws functioning only in social circumstances. The great importance of "spiritual" (*dukhovnye*) needs, e.g., esthetic or cognitive, is emphasized. These "higher" needs are characterized as products of social life. The question of how these needs are interiorized by individuals is not discussed. The author distinguishes between needs, motives, instigators, goals, etc. He suggests that a person's consciousness of his own motives may be useful to him. To the reviewer this recommendation appears to be inconsistent with the author's concept of "motive" which apparently includes consciousness by definition. A distinction between motives which do and do not lead to activity is made. Interests are characterized in effect as particular preferences with respect to cognitive activities. A considerable amount of attention is devoted to problems of motivation in educational situations. An amusing finding by Bozhovich is cited: the tasks disliked the most by children entering grade school are free assignments like "Draw whatever you like."

The chapter on speech (by Zhinkin) impresses this reviewer as a very informative one. A considerable variety of topics are discussed, e.g., the different functions of speech in interpersonal communication, the physics of speech sounds, the physiology of production of speech sounds, the determination of word meanings by their verbal context, the development of speech in childhood, the function of internal speech in thinking, etc.

The three chapters on child development (by Elkonin, with assistance of others) are an essentially unusual feature of the new textbook. The first begins with a characterization of the immature nervous system of the infant. It is stated that the development of the child is not "fatalistically" determined by heredity.¹ Characteristics of conditioned reflexes in infancy are described. They suggest a diffuse kind of nervous functioning and a preponderance of excitation over inhibition. As the child grows older, inhibitory processes develop, the nervous system functions in a more differentiated manner, and the speech (second signal) system develops and increasingly

¹ A 1936 resolution of the Central Committee of the Communist Party is cited as stating this.

becomes the dominant factor in learning. The child becomes increasingly accessible to verbal instruction. Most of the remainder of the chapter is devoted to a discussion of the socialization of the child in the course of interaction with grown-ups.

The facts of development are discussed in more detail in the two additional chapters, one devoted to preschool development,² the other to development during school years. The facts cited in these chapters have to do mainly with sensory development, increased differentiation of behavior with respect to different objects, various factors in socialization, the influence of school and organizational "collectives" (i.e., social groups) on development, and the influence of work and play on development. Ideas of Freudian origin about possible significance of feeding, toilet training, etc. are not mentioned. The existence of sexual emotions at puberty ("first love") is briefly discussed, with a statement that love relationships among young people should involve mutual respect. Problems which receive much more attention in this book than in American books have to do with the development of theoretical interests in youth, and with the influence of literary heroes on development of young people.

Two additional items will be noted about the book: (a) In the chapter on personality ["Kharacter" (Chap. 17) by Krutetsky] there is a passage on the role of the family in the moral upbringing of children; no such passages were noticed in the textbooks reviewed a year ago, although there were some in other writings reviewed then. It is stated in the book that in the Soviet Union the prime responsibility for moral upbringing rests with the school. But the family should cooperate with the aims of the school, and a congenial, friendly emotional atmosphere in the family is treated as important. It is also important that the parents should place appropriate demands on the child. (b) A considerable amount of attention is devoted to the ideas of the noted Soviet educator Makarenko (1888-1939). Makarenko's ideas were developed in his work with juvenile delinquents. He emphasized the great importance of the social group ("collective") for developing individuals. He appears to have developed techniques for dealing with "collectives" and to feel that educators should deal primarily with collectives (e.g., at class discussion meetings) and that individuals should be generally influenced through their collectives.

Proceedings of the Third Conference on Psychology.—At the Third Conference on Psychology, which took place in 1955 (4),³ a much larger number of papers was presented than at the Second Conference which had taken place two years earlier. Ninety-seven papers were presented instead of 31. More groups of topics were covered at the Third Conference than at the earlier one; thus one group of papers could now be devoted to abnormal psychology, another one to animal psychology. In view of limita-

² In the U.S.S.R., school attendance begins at age seven.

³ Some of the papers presented at the Conference have been published elsewhere before the publication of its Proceedings and were included in last year's review.

tions of available space, only a sample of the papers will be reviewed here, with an emphasis on those which contain relatively new material.

As was the case earlier, the largest number of papers in these *Proceedings* deals with problems of mastery of educational material by children. There are papers on the reading of topographical plans, on the understanding of drawings in perspective, on the understanding of grammatical categories, on stages in re-creative imagination of literary description, etc. In some instances, educational findings are used as starting points for theoretical considerations. Practically all of the papers in Parts IV, VI, VII, and VIII of the book deal with educational topics. In general, this material will not be reviewed; the papers tend to deal with topics similar to (in some instances, the same as) those covered in last year's review. Only two of the educational papers will be summarized, because of their relative emphasis on theoretical issues.

Galperin (5) discusses the theoretical implications of his three-stage method of teaching children scientific concepts* (1, p. 467). He states that the success of the method suggests that recurrent confusion between pre-scientific and scientific concepts should be viewed as a result of the commonly used teaching methods rather than as an intrinsic characteristic of childhood. When his method of instruction was used, mastery of concepts like "right angle" and "subject of a sentence" was very fast. No confusion resulted, except in experiments in which one or more stages was (were) omitted or abbreviated. The author interprets the mastery of a concept as the acquisition of the dynamic stereotype of a sequence of intellectual actions which have become generalized, abbreviated, and automatized.

Lublinskaya (6) discusses the perceptual and intellectual limitations of first-grade pupils. The study involved observation of children and their teachers both in classrooms and during experimental individual instructional sessions. Children are reported to be apt to confuse shapes with their mirror images, to commit errors in spelling in which features of earlier words intrude into words following them, to copy items in the wrong sequence, and to jump at conclusions based only on the most salient features of the problem. The author makes use of a number of concepts in interpreting these (and other) errors. She states that nervous processes of small children are characterized by high excitability, inertia, diffuseness, and deficient inhibition. This leads to low ability to differentiate items from each other, and to the tendency to perceive wholes without clearly differentiated parts (i.e., to synthesis without analysis), and to jump at conclusions (i.e., proceeding in problem solving in terms of "short association sequences"). The last mentioned tendency is likely to be reinforced by premature questioning of pupils by some teachers. The more successful teachers are char-

*First stage: give the child a printed list of criteria for deciding whether the concept applies to given objects and let the child use the list in making such decisions. Second stage: after the child tends to stop looking at the printed list, let him proceed orally. Third stage: child proceeds "mentally."

acterized as training their pupils in careful analysis and synthesis, by constant shifting from consideration of wholes to their parts to relations between parts. The author states that the superiority of such an instructional method over other methods is not always immediately apparent; but it becomes very noticeable in the second half of the first school year.

A number of papers deal with problems of moral development and upbringing of children. Thus Bodalev (7) deals with the development of the demands placed by school boys upon themselves in various areas of activity. A high level of demands is viewed as resulting from appropriately high demands by family, teachers, and class collective, appropriate support and encouragement, and success in meeting the demands. Four illustrative case histories are presented.

Asnin (8) discusses the development of self discipline in school children, partly on the basis of classroom and class meeting observations, and partly on the basis of results of remedial teaching and observation of school children through one-way vision screens. The author states that children believed to be lazy and weak-willed lack primarily in self-discipline. Self-discipline results from demands placed upon the child. It is likely to develop in stages. In the first stage, the child follows detailed instruction; after some intervening steps, the child develops the skill and acquires the habit of concentrating during work. But the demands of a teacher are not likely to be effective if they are contradicted by reality. Thus a child who is told to sit quietly during classwork because otherwise his work is certain to suffer, yet who does not sit quietly and still receives a good grade for his work, is not likely to take the demand to sit quietly seriously. Sometimes the quality standards for the school work have to be raised so as to make successful performance unlikely unless pupils behave with self control and discipline.

A paper by Rubtsova (9) deals with development of moral ideas in school, mainly on the basis of children's evaluation of literary characters; but the author also utilized children's discussions of each other's behavior at class meetings, and examined some diaries. The author reports that moral judgment of children in the primary grades tends to be based on overt action and its outcome, rather than on intent and feelings. When feelings are considered in moral valuation children are apt to misuse them; thus children tend to characterize a person who performed a heroic act while afraid as a coward. In the middle school grades, moral evaluation is more likely to be based on intent and feelings of the person than it is in the primary school grades. In the last years of school attendance, moral evaluations tend to become systematized and to be related to needs towards self-understanding and the understanding of others.

There are several additional papers dealing with more or less similar topics. However, they tend to be reviews of large numbers of research projects or expressions of the authors' opinions and contain little concrete material. In some instances considerations related to communist ideology

enter into the picture. Thus in Kovalev's (10) paper on character types, three "types" of youth are discussed: (a) youth with communist convictions, strong ties to society and a definite life plan; (b) individualistic youth motivated by selfish considerations;⁵ (c) youth who appear to accept the demands of their collective, but who do not exert sufficiently strong efforts in meeting them. Types (a) and (c) are divided by the author into subtypes, related to Pavlovian types of higher nervous activity. It should be noted that throughout the group of papers just characterized there are numerous references to the work of Makarenko.

Another group of papers deal with "physiological mechanisms of psychical activity." A paper by Boiko *et al.* (11), which deals largely with material like that reviewed last year (1, p. 489), contains a new theoretical feature. The authors object to too wide interpretations of the conditioned reflex. They state that the functioning of any conditioned reflex is only reproduction of previously established bonds, and cannot explain intellectual creativity. Intellectual functioning involves a different process, that of interaction between different conditioned reflexes. Sechenov is named as the originator of the idea of interaction between functions acquired separately.⁶

A paper by Dolin (12) reports the results of a group of investigations of the early stages of the conditioning process. The experiments included the production of conditioned cataleptic states, of conditioned changes of properties of the subjects' breathing, of pulse rates, of the disturbance of dark adaptation, etc. The author reports success in his conditioned reflex experiments with all of these functions. He presents evidence which indicates that, in an early stage of the conditioning process, the conditioned reaction is likely to be the opposite of the unconditioned one; thus, when the unconditioned reaction is an increased pulse rate, one is first likely to obtain conditioned decreases of the pulse rate, and the reaction becomes similar to the unconditioned one only later in the conditioning process. In the case of the more complex reflexes, the conditioning of their components is apt to proceed at different rates, so that the response pattern may at times be quite different from that of the unconditioned reflex. The unconditioned reflex is also likely to change during the conditioning process. It tends to become "corticalized." In support of this inference findings with unconditioned reflexes which resemble those on dynamic stereotypes of conditioned reflexes are cited [See (1, p. 459)]. The stages of formation of conditioned reflexes are likely to be much shortened by the use of appropriate verbal instructions.

Markosian (13) investigated the possibility of obtaining conditioned changes of the properties of blood contributing to its rapid coagulation. Both humans and animals (it is not clear, what kind) were used as Ss. A painful electric shock was the unconditioned stimulus, the sound of a metronome the conditioned stimulus in most of the experiments. The experiments were

⁵ They are said to be rare.

⁶ In the earlier paper, Boiko credited the idea to Ivanov-Smolensky.

reported to have been successful. Blood samples withdrawn after shocks coagulated faster than samples collected without preliminary shocks. After a few combined metronome-shock presentations, the metronome sound alone produced effects on blood coagulation similar to those resulting from the shock. Instead of the sound of the metronome, the spoken word "metronome" or more or less similarly sounding words (e.g., "microtome" or "microscope") could be substituted. Generalization in accordance with similarity of meaning was also reported. The author concludes that the cerebral cortex is capable of affecting phylogenetically ancient functions which contribute to blood chemistry; even the speech system plays an important role in this cortical control of biochemical functioning.

Several papers deal with problems of perception. A paper by Ananiev (14) is a review of work concerned with tactual and kinesthetic perception of the hand, done by him and his associates over a period of some 20 years. There is much Marxist-Leninist theorizing in the paper. Some of the reported findings are: When an object is moved by an experimenter on a S's stationary hand, S tends to recognize the shape of the object fairly accurately, considerably better than he can recognize it when both the hand and the object are stationary. This indicates that some of the generally known superiority of the moving hand over the stationary hand in exploring shapes of objects is due to the relative displacement between hand and object rather than, as is often believed, wholly to the presence of kinesthetic impulses in the case of the moving hand.

If an object is given to a subject for two-handed exploration, the hand movements tend to be synchronous if the object is symmetrical, asynchronous if asymmetrical. If the subject is made to proceed synchronously with an asymmetrical object, phenomena similar to binocular rivalry are often reported. In right-handers, kinesthetic impulses tend to dominate over tactual ones in the case of the right hand, while the opposite is true of the left hand. Also, in two-handed examination of objects, when the right hand is the leading one with respect to kinesthesia, the sense of touch is often better developed in the left hand. The author attempts to explain this "contradictory" state of things in terms of elaborations of Engels's hypothesis of the role of work in the evolution of man.

Hellerstein (15) reports experiments in which Ss were required to estimate and control their reaction times (apparently in hundredths of seconds). The training went through several stages: participating in reaction time experiments; being told one's reaction time immediately after each experiment; being told one's reaction time after first guessing it; being asked to speed up or slow down one's reaction time; being told to give similar assignments to oneself; and, finally, to react in assigned numbers of hundredths of a second. The author reports that at the end of the training Ss estimated times correctly (apparently, within .01 sec.) and were able to produce assigned reaction times in over 80 per cent of the cases. The reaction time was markedly speeded up. The Ss had developed a highly dif-

ferentiated "sense of time" for short time intervals. Additional experiments indicated that this "sense of time" has a multiple sensory basis.

Golubeva (16) reports on results of measurements of the visual threshold in dark-adapted Ss after dark adaptation is disturbed by added visual stimulation (added light in the room) of from a few seconds to half a minute. Such added stimulation is likely to result in a rise of the threshold; but the rise tends to decrease if the experiment is repeated. The diminution of the disturbance of dark adaptation is explained by the author as being due to extinction of the "orientational reflex" (see 1, pp. 473, 484) to the added visual stimulation which thus ceases to be fully effective. In part, the extinction of the orientational reflex is explained as being due to the fact that the added light is shortly followed by darkness so that a "conditioned bond" is established. Additional experiments are cited in support of these interpretations. Experiments making use of electroencephalograms (EEGs) are also reported.

Zimkina (17) investigated the effects of sensory stimuli on breathing changes, vascular changes, and galvanic skin responses (GSRs), viewed as components of the orientational reflex. These components are apt to appear at different times and to develop at different rates (The patterns of these differences varied from subject to subject). Similar differences are reported for the extinction of the components of the orientational reflex. The author discusses the great complexity of the reflex. Some of the experiments reported in the paper deal with orientational reflexes of the blind, of neurotics, and of brain-injured (without overt symptoms) patients. In the two latter groups, the orientational reactions to stimulation tended to resemble those which normally occur in response to threat.

Lomov (18) investigated certain generalization and differentiation phenomena in conditioning, when words and drawings were substituted for objects (cube, pyramid, etc.) functioning as conditioned stimuli. The findings indicated that the drawing is closer in its physiological effects to "signals" of the first (perceptual) system than to those of the second (speech) system.

Presumably as a result of the emphasis on afferent processes in Soviet psychology, a paper by Tamuridi (19) on the development of handedness in babies is classified under "sensation and perception." The "functional asymmetry of the motor analyzer"⁷ is measured by the frequency with which the baby touches (or grasps) a toy offered to him with one or the other hand. Early appearance of tendencies towards handedness is reported. In view of the rather slight degree of hand preference in early age, the author expresses the belief that righthandedness should be encouraged, and that this can be accomplished by consistently placing toys near the baby's right hand and using other similarly gentle procedures.

Two groups of papers are classified in the *Proceedings* as dealing with

⁷ These words occur in the title of the paper.

various aspects of speech. The first group of these papers mainly deals with educational processes. The second group of papers deals with physiological aspects of speech, or with the role of speech in control of motor or intellectual activity. A paper by Bassin & Bein (20) reports on muscular action currents (mostly from the lower lip and a finger) during the performance of tasks involving inner speech. Normal people, aphasics and cases of hysterical mutism were used as subjects. Characteristic differences were reported. Thus in normal subjects increase in the difficulty of the task resulted in moderate increases of the intensity of lip action currents; in aphasics, the initial increase of the lip action current tended to be much larger, but tended to be followed by a sharp drop as the difficulty of the task was increased further; in cases of hysteria, there tended to be irregular, unpredictable relations between the difficulty of the task and intensity of action currents. The authors discuss the diagnostic and prognostic uses and the theoretical significance of their findings.

A study by Novikova (21) deals with action currents from the tongue during the performance of intellectual tasks. Such action currents regularly occur and increase with the difficulty of the task. With verbal and arithmetical tasks the action currents are more intense in semiliterate subjects than in literate people. In the deaf, tongue-action currents tend to be synchronized with finger-action currents. In the teachers of the deaf, who are familiar with sign language, functional systems involving the musculature of the hand and the speech organs are also found, but the speech organs play the leading role.

Sokolov (22) investigated the role of speech functions as components of a number of performances: memory for words and drawings, the solution of arithmetical problems, etc. Tongue movements were recorded during these tasks; speech movements were interfered with; and extraneous competing speech tasks were introduced. The various experiments indicated that speech was a component of the perceptual, memory, and intellectual performances which were studied. But the interference with the task was only slight when speech movements were mechanically prevented. The author argues that trace effects of past speech involvement may play a role in absence of current speech functioning, pointing out that speech is a complex, meaningful performance. He refers unfavorably to Watson's verbal-motor theory of thinking, which he also attributes to Lashley and to Max.

One group of papers is classified as dealing with psychology of work, sport, and practiced motor skills. Actually the papers generally deal not so much with motor phenomena as with aspects of cognitive functions during motor performances, and in many instances are concerned with laboratory tasks rather than sports or work operations. A study of Bogdanova (23) deals with learning to perform an arm movement of a given amplitude and sometimes also of a given duration. The author found that initially there is little consciousness of error or of relevant sensory cues. In a second stage of the mastery of the skill the subject becomes conscious of many sensory

cues, both relevant and irrelevant. The number of unnoticed errors (and therefore of total errors) decreases rapidly. In the third stage the irrelevant sensory data cease to be noticed and practically all errors are recognized.

Gagaeva (24) reports on the effects of certain verbal functions on the learning of laboratory tasks and athletic skills. One laboratory experiment involved the measurement of choice reaction time. In some of the experiments, the subjects were asked to count silently during the experiments. The silent counting slowed down the reaction time but only before the subjects had acquired sufficient practice in reacting. The author suggests that counting interferes with self-commands (which she views as essential in the early stages of the learning process). In another experiment, the author found that learning to produce active movements of the same length as passive movements is improved if information about the length of the passive movement is verbally presented to the subject. In view of the apparent usefulness of verbal material in the two laboratory tasks the author developed a training technique in gymnastic performances in which the trainees were to describe after an exercise how the exercise ought to be performed. Errors in the account were pointed out, after which the subject had to repeat the exercise. The author obtained good results with 24 out of 27 students of physical education and reports successful application of variations of the method by outstanding athletes.

Gozova (25) reported on the wide distribution of attention of factory workers who have to watch several weaving machines operating simultaneously. The paper appears to be based on observations and interviews. Multiple sensory control of the work is reported. The improper functioning of a machine is often recognized by hearing. The proper time to put in new thread is likely to be anticipated by the worker in terms of passage of time. An efficient, experienced worker can take care of as many as sixteen machines simultaneously. The task becomes a complex, unified one, and successful workers do not find it difficult or tiring.

Several papers are classified as dealing with the problem of set. Two of these papers were mentioned in last year's review, but only on the basis of incomplete information. One of these papers, by Bzhalava (26), deals with characteristics of Uznadze illusions⁸ of schizophrenics and epileptics, and with some other experimental findings. Four-hundred-fifty epileptics and 205 schizophrenics were the subjects. A large number of results, often striking ones, were obtained. Some of them were: In epileptics, visual Uznadze illusions could be obtained only seldom (16.5 per cent), while in the case of balls placed in the hand they were present in 98 per cent of the cases. In schizophrenics, both visual and tactual presentation of the appropriate stimuli produced the illusion.⁹ For epileptics, the illusion

⁸ The illusion is one of after-effect of perceived objects of different size on the perception of equal objects [See (1, pp. 472)].

⁹ This statement is based on the author's text; a percentage in a table seems to contradict it, but may be a misprint.

was "local"; i.e., if it was established for one sense modality, it failed to affect other sense modalities. For schizophrenics, there tended to be complete transfer from one sense modality to another. For epileptics, it was found to be possible to produce tactual illusions of size by asking Ss to imagine an object of different size in each hand; this experiment does not succeed with schizophrenics. In both epileptics and schizophrenics, the illusion is particularly easily established, is usually very persistent, is peculiarly resistant to extinction, and fails during the extinction process to pass through the normally occurring stage of reversal of the illusion. Additional experiments were conducted, making use of words and nonsense syllables made up of ambiguous letters with one meaning in the Russian, another in the Latin alphabet [see (1, p. 484)]. All the experiments are interpreted in terms of postulated properties of sets of schizophrenics and epileptics; theories of mental functioning in schizophrenia and epilepsy are offered.

Norakidze (27) investigated the Uznadze illusions in relation to the Pavlovian temperaments. On the basis of biographical information, clinical interviews, etc., 59 subjects were selected from an original 153 as representing "comparatively pure types" for experimental study.¹⁰ In the "melancholics" ($n = 9$), the Uznadze illusions tended to function like in the schizophrenics studied by Bzhalava¹¹. The illusion was very quick to appear; there were effects on other sense modalities; the illusion was stable and hard to extinguish. In the "sanguinics" ($n = 20$), the illusion tended to be fairly quick to appear, to reach a maximum gradually, to be extinguished rather readily, and to "irradiate" into other sense modalities. In the "flegmatics" ($n = 10$), the illusion tended to appear and disappear slowly, and there was little fluctuation in the illusion from trial to trial. In the "choleries" ($n = 20$), the illusion tended to appear quickly and disappear slowly, and there were few inter-sense-modality transfer effects.

A group of papers is classified as dealing with "pathopsychology and defectology." A paper by Zachevitsky & Yakovleva (28) deals in much greater detail with material also presented by the authors elsewhere and summarized in last year's review. The author includes five case histories of neurotics; the neuroses are attributed to improper upbringing, which is viewed as failing to train the nervous functions. One of the cases was that of an adult hysteric. His personality makeup (egocentricity, pretentiousness, and lack of effort in overcoming difficulties) and symptoms are traced back to childhood during which he was indulgently overprotected (this term of Levy is not used), was admired to an excessive degree by his parents, and successfully used temper tantrums to obtain what he wanted. The intense shyness and social backwardness of a young girl (an agricultural technician) is traced to feelings of unworthiness fostered in her by her parents. When her parents quarreled, the mother tended to accuse the father of

¹⁰ As is generally the case in the Soviet Union, the author takes the reality of types for granted.

¹¹ The author does not point out the similarity.

having produced a daughter as bad as he, with the father retaliating in kind. The parents restricted her play with children, did not give her any household responsibilities, and tended to make decisions for her. Her initial school adjustment was relatively good, but her intense shyness spread to the school situation after some scoldings by one teacher.

Zemtsova (29) studied the development of recognition of models (and raised images) of objects by touch in 8- to 12-year-old blind children. She found that children in the primary grades perceive objects by touch only very imperfectly and feels that the "organizing role of the word" is a very important factor in enabling children to perceive objects and that the selective perception of details is related to utilization of the objects in action in the past.

Kogan (30) discusses observations of disturbances of cognitive functioning in aphasics, including motor aphasics. Only examples of the observations can be given here. Thus motor aphasics are apt to be able to classify pictures of objects only according to one principle (e.g., table and chair are picked out as belonging together), but are not able to utilize the sentences "Table, chair, bed and cupboard are all furniture" or "A table for a mechanic's work is called a work bench" in classifying the pictures of these objects as belonging together. A motor aphasic is reported to have been well-satisfied with the following "explanation" of the difference between an apple and a ball: "An apple is round, but a ball bounces." The author utilizes these and other observations in a theoretical discussion of the nature of cognitive disturbances in aphasia. The discussion is too complicated and too condensed for an adequate summary. Ideas of Head, Goldstein, Poetzl, Jackson, Marie, etc. are mentioned. One of the author's statements is: "From our point of view, the essence of the disturbances of speech in aphasia has to be searched for in the disruption of the system of concepts, in which is included the potential transformation of the many-valued content of a word into a concrete significance."

Rubinstein (31) infers from what she calls the reflex theory of psychic life (the point of view that psychic life begins with stimulation and involves activity of the organism) that the traditional distinction between illusions and hallucinations may not be justified. So-called hallucinations may be in reality illusions which occur in response to stimuli the psychiatrist has not noticed. In order to test this interpretation, the author prepared tape recordings of sounds, e.g., of crumpling paper, street noises, sobbing, passage of air bubbles through water, etc. The tapes were played to patients diagnosed as having hallucinatory paranoid reactive conditions. The tapes were played at low intensity, so that the experimenter could hear them but an outsider coming in without being told about them would be unlikely to notice them. The patients reacted to the sounds with illusions and associations to the illusions which a visitor uninformed about the experiment would have mistaken for hallucinations.

In a paper by Khvilivitsky (32), pharmacological experiments with

psychotics are reported. In several cases, manifest anxiety or depression was markedly relieved; but delusions, which are often viewed as secondary to the emotional factors, remained unchanged.

The last group of papers deal with animal behavior. A paper by Gerd (33) presents information about methods of training of circus animals in the organization founded by the animal trainer Durov. The methods are examined in relation to conditioning theory. The training is accomplished by the use of rewards (no punishments), which are treated as reinforcements. Methods are described for initial acquisition of tricks by animals, and also for their later stabilization by occasional, not too infrequent, reinforcement.

Ladygina-Kohts (34) presents material on the utilization of objects by an adult male chimpanzee observed by her over a period of five years. The author describes the ape's utilization of materials and its fine discrimination between their textures in nest building. Nest building is viewed as instinctive, i.e., as a complex unconditioned reflex; but it is also flexible and adaptive; the ape makes the necessary adjustments if various materials are offered to it in the wrong sequence. Apart from nest building, the ape engages in little constructive activity, although the animal uses objects for many purposes. The objects are not treated as permanent and are readily destroyed. The author views the very limited amount of constructive activity (outside of nest building), the fine sensory discrimination, and the tendency to take objects apart as related to the circumstances of the life of the species in its natural habitat. She characterizes the thinking of the apes as qualitatively different from human thinking. The difference is based on the weakness of trace excitations (images) in the ape, which leads (among other consequences) to the absence of true concepts; as a result, "spatial-temporal bonds developing in the chimpanzee are not transformed into those of cause-effect" (p. 700).

Proceedings of additional scientific conferences.—The proceedings of three additional recent scientific meetings were examined. One of these was a local meeting of Ukrainian psychiatrists which took place in 1954 (35). Most of the papers appear to be of no interest to psychologists. One of the exceptions is a paper by Frumkin (36) commemorating the hundredth anniversary of the birth of the noted Russian psychiatrist, Korsakov. Korsakov is praised for a number of his achievements and attitudes. These included his materialism, his approval of experimental psychology, his view that the organism is a whole and should be so treated in disease, and his recognition of the role of personality differences in mental disorders. His humanitarian attitude towards patients and his emphatic advocacy (and consistent use) of the no-restraint policy are discussed favorably, as are his contributions to the classification of psychiatric disorders. With respect to the last, Korsakov is praised as having anticipated some of Kraepelin's work on what eventually became known as schizophrenia.

Zaviliansky's paper (37) deals with psychotherapy of schizophrenia. He

states that already in 1951 the desirability of wider recognition of the use of words as a therapeutic agent received considerable attention at meetings of ruling boards of medical and psychiatric organizations. The forms of psychotherapy which were discussed included hypnosis and "so-called clarifying speech therapy." The nature of "clarifying speech therapy" is not explained, but in a discussion of a case it is mentioned that contact with the patient was established, that symptoms were discussed, and that the patient came to recognize the fact of his illness. It is not clear what else may have been discussed. The author states that "generally known is the significance of psychogenic factors in early schizophrenia." But these factors are not discussed. The paper (as others which are still to be mentioned) is somewhat less noncommittal about psychological factors in mental disorders and therapy than the publications reviewed a year ago.

The proceedings of a very much larger psychiatric meeting were reported in 1955 (38). Almost a hundred papers are included. Most of them are not relevant to psychology, but are concerned with purely medical approaches to mental disorders, the history of psychiatry in Russia, etc. A substantial number of papers deal with the use of motor conditioning with verbal reinforcement in research on psychopathology. These papers will not be reviewed, for several reasons: examples of such research were presented in last year's review; the findings of different studies do not agree with each other; no control groups seem to have been used in any of them; the significance of supposedly abnormal results (e.g., failure to obtain motor conditioning with psychotics) is obscure, because such results have been reported in other Russian sources as common in normal Ss.

The following items presented at the meeting were chosen for inclusion in this review: Alexandrovsky (39) characterizes theories which neglect the role of environmental factors in schizophrenia as "Morgan-Weissmanism" (which is a term of opprobrium in the Soviet Union). He thereby clears the way for a theory in which prolonged chronic traumatization is treated as one of the causes of schizophrenia, together with susceptibility determined by heredity or environment. The prolonged traumatization results in exhaustion of the cortical cells and "phasal" phenomena. The author characterizes his point of view as true Pavlovianism.

Ambrumova (40) discusses a number of cases of schizophrenia which started as reactive psychoses. All of them were originally diagnosed as reactive psychoses. The onset in each case followed some trauma, and initially the content of the symptoms was appropriate to the trauma.

Fedorov (41) cites Pavlov's view that schizophrenics tend to have a "weak type" of higher nervous activity. He reports that according to his findings, the life history method confirms Pavlov's opinion in the majority (60 per cent) of the schizophrenics. Normal people and patients afflicted with reactive disorders are said by the author to have mostly strong types of nervous functioning. The weakness of nervous functioning in schizophrenics leads, in Pavlov's terms, to transmarginal inhibition.

Orlovskaya (42) discusses the onset of schizophrenia. The clinical observations are not new but they are interpreted in terms of Pavlov's theories and placed in relation to findings on motor conditioning with verbal reinforcement. Thus the neurosis-like state with depressed feeling tone and tendency to overreact to minor grievances, common in early schizophrenia, is viewed as related to the paradoxical phase, in which weak stimuli are apt to lead to stronger responses than strong stimuli. The return of early memories in schizophrenia is interpreted in terms of inhibition, disinhibition, etc.

Similar ideas are suggested by discussions in some other papers. Thus Zykova (43) says that the state in late schizophrenia which is characterized by delusions, illusions, and hallucinations generally also involves prolonged insomnia and represents a somewhat sleep-like state. Sleep is generally viewed in Russian science as a state of widespread cortical inhibition. Schizophrenic confusions between, e.g., friends and strangers are characterized by the author as response reversals such as occur in the ultraparadoxical phase.

Several papers deal with the widespread physiological responses to sensory stimulation (often referred to as the "orientational reflex" in Russia) and their disturbances in psychotics. Thus Stanishevskaya (44) reports on breathing curves and plethysmographic records of schizophrenics obtained both with S at rest and while he is being subjected to a number of mild stimuli (warmth, cold, words "applying warmth" and "applying cold", colors, sour tasting substances). In the early stages of the disease the results were very variable, even on the same day. In stuporous patients reactions to the stimuli were reported as weak or absent; some abnormal shapes of breathing curves were observed. In deteriorated schizophrenics the results were variable. Sometimes the reactions to the stimuli were weak or absent, and there were some bizarre shapes of breathing curves. Work in related areas was also conducted by Sarma (45) on breathing curves and by Streltsova (46) on the pupillary reflex to various nonvisual stimuli. Both authors indicate that abnormal experimental results have an unfavorable prognostic significance.

A meeting devoted to physiology of work took place in 1953. The proceedings of the meeting (47), consisting of 22 papers, were published in 1957. Examples of the reported findings are: Kosilov (48) describes a variety of results on the work process and on experiments on motor conditioning with verbal reinforcement during the working day. He states that, in conveyor work, if faster work is made necessary by an increase of the speed of the conveyer, the speed-up tends to occur at the expense of the pauses, rather than the work movements. In fatigue, the work movements tend to slow down, and the pauses tend to shorten. Thus the work rhythm is disturbed, and this disturbance occurs before there is any loss in output. The author suggests that this disturbance of the work rhythm is a good indicator of the need for rest pauses. The author recommends regularly

scheduled changes in conveyer speed in order to preserve the worker's dynamic stereotype of work rhythm. The author also reports that motor conditioning,¹² differentiation, and extinction experiments indicate increased inhibition toward the end of the working day, together with "phasal states" or disturbed effects of increased intensity of stimulation.

Volkov, Babajanian & Kostina (49) investigated various physiological changes (e.g., of auditory thresholds, ECGs) in various categories of railroad workers, e.g., locomotive engineers and firemen, dispatchers, etc. The authors report increases of the auditory thresholds to sounds with vibration frequencies corresponding to those present in the noise to which the worker has been exposed for a long period. The auditory threshold changes are influenced by the nature of the task at hand; the changes are different in locomotive engineers, who have to respond to the sounds of the engine, and firemen, who do not. A complex pattern of physiological changes after work is reported in the case of dispatchers.

Slonim (50) reports on a variety of problems related to the topics of fatigue and rest. One of these has to do with "active rest", i.e., resting while engaged in relatively effortless activity. The author characterizes active rest as an efficient method of resting. The author also reports on various situations where there has been conditioning to the time of the day. He states that a number of functions tend to become so conditioned, and form a complex dynamic stereotype. Both work operations and sleep tend to become components of the dynamic stereotype, and the functioning of both depends on a regular schedule. The schedule need not be the conventional one. Locomotive engineers who drive locomotives long distances at night learn to sleep at unusual times and places without any ill effects, but only if the opportunities for sleep are sufficient and regular. If not, sleep becomes poor and work efficiency suffers.

Leinik (51) describes a complex cycle of four stages of rest. In the first stage, strength is low but rises and fatigability is high. In the second stage, strength is higher than before work but fatigability is still high. In the third and fourth stages, fatigability is low. In the third stage strength is normal; in the fourth stage it becomes somewhat subnormal. The author draws the appropriate inferences about work and rest schedules.

Books on psychology of sports.—A number of books on varied psychological and related topics were examined since the collection of the material for last year's review. One of these books (52) is a collection of papers on psychology of sports. The book appeared in 1955 and contains the irrelevant references to Stalin and the Marxist-Leninist theory of consciousness as reflection of reality,¹³ which used to be very common in Soviet psychology. The book was listed as available in this country a year ago.

¹² The experiments on motor conditioning with verbal reinforcement, together with experiments on differentiation and extinction, are often cited in the book as indicators of the condition of excitatory and inhibitory brain processes.

¹³ In a paper on the conscious experience of people learning high jumping.

but could not be obtained at that time. This reviewer found the book to be concerned largely with the conscious experience of athletes, particularly as far as the athletic performance is concerned.

The first paper (by Puni) is a general one and deals (partly on the basis of theoretical considerations) with the theory of consciousness in skills. The other papers deal with high jumping, gymnastics (two papers), tennis, skiing (slalom—two papers), foot racing. The research mainly consisted of questioning athletes after regular or specially arranged athletic contests. In some instances specially designed methods of instruction were tried out.

Tennis players (Abelskaya, 53) commonly talk to themselves during tennis matches. They give themselves commands during play (e.g., "to the backhand", "hit hard"), formulate approval or disapproval of their own play or that of their opponents, evaluate the play situation, and make plans. The plans of the better players tend to be more detailed and tend to be based on fuller evaluation of the match situation than plans of poorer players.

Slalom racers of championship class remember the courses seen before the race better than the less efficient ones. The racers who do well in a particular race remember the course better than those who do poorly in it (Egupov, 54).

Two physical education students who were taught high jumping by the usual method of "analysis and advice" by their instructor progressed less regularly than two others who were (in addition to the customary method) urged to imagine the details of their own jumping before the start of each jump. The superiority of the latter group was delayed rather than immediate. The subjects who were instructed to imagine their high jumps tended to be more aware of the details of their jumps than the other subjects. The authors do not appear to be disturbed by the fact that they had only four Ss in this part of their study (Abelskaya & Surkov, 55).

Another book on sports (56) deals, as its title suggests, with relations between athletic training and the Pavlovian doctrine of higher nervous activity. Actually, the first half of the book deals partly with Pavlov's own participation in sports until a very old age, and then presents an account of Pavlov's general ideas on neurophysiology. The second half of the book presents applications of Pavlovian ideas to sports. It provides an additional illustration of the fact that, in Russia, conditioned reflexes are not viewed as blind habits. The emphasis in the book is not on repetitive drill. Rather this book deals with, e.g., the need of the athlete for sufficient rest, because otherwise the systems of conditioned reflexes are apt to be disturbed. Emotions are treated as important factors in athletic performance in several ways. A "pep talk" by a coach the night before an important game may interfere with sleep (ultraparadoxical phase) and thus disrupt a team's play. Conditioned anxiety following a failure or an accident (e.g., a pole breaking during pole vaulting) may set up inhibitory effects which may require careful retraining. The importance of verbal instruction in athletic

performance is much emphasized and justified in terms of Pavlov's view of the leading role of the second signal system (compared to the first signal system) in man. A statement of Pavlov about ideomotor action is used to justify the emphasis on the need of athletes to have correct ideas about their skills.

The interest in Russian psychology in conscious experience is illustrated by the title of a book by a Russian deaf-blind woman, Skorokhodova, *How I Perceive and Imagine the Surrounding World* (57). The author was born in 1914 and she became blind and almost deaf in 1919, and totally deaf about four years later. In (or near) 1922 her mother died. Her father had been drafted into the Army during World War I and did not return. A few days before her mother's death she went to live for about half a year with an aunt; then she spent about three years in a school for the blind and in a sanatorium, but did not receive appropriate instruction until she was placed in a special institution for the deaf-blind in 1925 (which ended her life of helplessness and isolation). She learned a large number of practical skills (e.g., cooking), relearned speech, acquired secondary school education, and studied literature at the University of Moscow. According to information in the book, she is currently employed as a junior assistant at the Institute of Defectology and conducts research on the psychology of the blind.

In an introductory article, Sokoliansky, who was the director of the special institution at which Skorokhodova was educated, discusses a number of issues related to her case and other similar ones. He views the problem of education of the deaf-blind as one which has been satisfactorily solved, but does not describe the methods in detail. He only states that instruction in speech should not come too early, not before the deaf-blind has reestablished an adequate contact with his environment and is well-oriented in space and time. He points out that the fact that Skorokhodova lost her vision and hearing after learning to speak did not make the educational task any easier or the degree of her adjustment any less impressive. On the contrary, the author states that children who become deaf and blind comparatively late in life tend to lose their speech quickly, become helpless, and present particularly difficult educational problems. Without appropriate educational procedures they become invalids for life. With proper training, they achieve a normal, and sometimes a highly superior, intellectual level, like Keller, Skorokhodova, and another noted Soviet deaf-blind, the sculptor Kurbatov.

The main body of the book is a collection of notes by the author about her various experiences, episodes from her life, dreams, etc. There are also a number of poems written by the author. It is not possible to summarize the book. Instead, translations of a part of a well-known (in Russia) poem by the author and of an account of an episode will be presented as samples of the book. The nearly verbatim translation of the first half of the poem is:

Others think— those who hear sounds, those who see the sun, stars and moon:—
how will she describe beauty without vision?
how will she understand sounds and spring without hearing?

I shall hear a smell and the coolness of dew, I catch the light rustling of leaves.
Drowning in the dusk, I shall walk in the garden, and I am ready to dream and to
say, "I love."

Let me not see the shining of his eyes, not hear the caressing live voice.
But I catch and hear words without sound with my quick hand—the trembling of
feeling.

An example of an episode is:

I sat in the dining room and studied a physics lesson. D. walked over to me and softly took my hand. I looked at him [the author often uses the term "looked" instead of "examined"] with astonishment. I recognized his hand, but he smelled of eau-de-cologne. This was the first time that he smelled like that and therefore I did not recognize him at once. I thought, "But who is it?" D. repeated the hand shake and I recognized him. I smiled and frankly confessed, "I did not recognize you at once." (I was too embarrassed to speak about the eau-de-cologne smell.)

A book on psychotherapy.—In last year's review, it was mentioned that steps were being taken in the Soviet Union aimed at the introduction of courses in psychotherapy at medical schools. In 1957 a book by Platonov on psychotherapy appeared (58). The book is described as a second edition; but this is not quite correct as the material is largely new. The earlier version, which appeared in 1930, was only a long paper from a collection (59, pp. 11-122) while the book is almost four times as long as the paper. Platonov's book deals to a large extent with hypnosis. A Pavlovian interpretation of hypnosis is offered, mainly in terms of the concepts of conditioning, inhibition, and paradoxical phase.¹⁴ A considerable amount of attention is devoted to experiments (both by the author and by others) which demonstrate the physiological effectiveness of hypnosis. Some are very striking. For instance, it is reported that the sugar content of Ss' blood tended to fall after drinking concentrated sugar solution if they were told (in hypnosis) that they were drinking distilled water.

Therapeutic applications of hypnosis also receive much attention. The hypnotic state was used in some instances merely as a sleep-like state in which the subject could rest; when this was intended the subject was hypnotized, and then no further suggestions were given. More often suggestions aimed directly at the removal of symptoms were given. The author reports many successes with these methods in treating various conditions, including, e.g., alcoholism, various neurotic disorders (a number of which had been previously falsely diagnosed as of organic origin and incurable), psychoses

¹⁴ The paradoxical phase is used to explain why subjects tend to respond to the relatively weak verbal statements of the hypnotizer, rather than to the sensory stimuli which ordinarily have a much greater effect.

of psychogenic origin, etc. Even two instances of extramarital relations (and lack of sexual interest in the patient's spouse) were successfully treated by hypnotic suggestion. Contrary to what one might expect in terms of beliefs which are prevalent in this country, the author does not report any tendency of hypnotic cures to be transitory or to be followed by the appearance of new symptoms. On the contrary, many cures are explicitly characterized as lasting ones; the follow-up periods varied greatly, from just under one year to over 20 years.

But hypnosis is not the only psychotherapeutic method practiced by the author and his colleagues. The author states that, as a rule, psychotherapy should begin with detailed "anamnestic conversations" in which the past and present situations of the patient are explored; some of these conversations are characterized as affording the patient the opportunity to express himself freely while the therapist listens patiently. The author states that the mechanism which led to the neurosis should be utilized in its cure. Without thorough anamnestic conversations, hypnotic suggestion aiming directly at the disappearance of the symptoms often fails to succeed. Once the traumatic experience (or chain of experiences) which led to the neurosis is known, hypnotic suggestion aiming at removal of emotions associated with it (e.g., "You will forget the fear.") can be attempted.¹⁵ Anamnestic conversations are also characterized as sometimes beneficial in themselves; or they may serve as the basis for subsequent "clarifying" or "persuasive" therapy in the waking state. The nature of clarifying and persuasive therapy¹⁶ is explained only briefly. The following example (58, p. 216) may serve instead of an explanation: Patient D. suffered from intense headaches, depressed mood, seemingly unmotivated tears, insomnia, loss of interest in her studies. She was deserted by her husband in the fifth month of her pregnancy. She was ashamed of her pregnancy, thought of the care of the coming baby with horror, etc. She was told by the therapist that her symptoms were the result of her inability to accept her separation from her husband. But in reality the separation from the husband had been necessary and solved many problems for her which would have arisen in the future: since her husband proved his irresponsible nature by deserting her while she was pregnant she had no right to expect a satisfying common life with him. She would have the right to be proud and joyful after the birth of her child; the child would be her little friend; she would never be lonely again;

¹⁵ However, the author cites a case of "perversion of the maternal instinct" which was cured after seven sessions of hypnotic suggestion, although the cause was not discovered until 23 years later. The patient was a woman who suffered from an obsessive urge to choke her baby to death. The baby was her son by her second husband, whom she married to give her older boy (by her first husband) a father. She was fond of her second husband who "fulfilled her hopes," but was not sexually attracted to him. She was unwilling to have another child, lest the husband's affection to his step-child should change.

¹⁶ The author credits it to Dubois and Bekhterev.

she would receive adequate assistance in caring for the new baby, etc.

The author makes the distinction between "minor" and "major" psychotherapies. "Minor" psychotherapy is to be used when the traumatizing factors are clear to both the therapist and the patient. It consists of encouragement, some persuasion, suggestion, purely symptomatic therapy. "Major" psychotherapy involves combined use of several psychotherapeutic methods, including "more or less deepened" anamnestic conversations, persuasion, and various forms of suggestion. It should be noted that in 1930 the author referred to psychoanalysis as one of the scientifically legitimate forms of major psychotherapy. He also characterized anamnestic conversations as a form of minor psychoanalysis. In the 1957 volume, he severely criticizes psychoanalytic ideas, partly on political and philosophical grounds, as reactionary and idealistic; he also objects to psychoanalytic neglect of social factors in neurosis, to the theory of the "necessary connection" between neurosis and infantile sexual trauma, etc. Three cases of patients who did not benefit from psychoanalysis (two years to several months), but who were cured by the author's methods in a few weeks, are cited. The author's theoretical interpretation of neuroses is mainly in terms of assumed disruption of higher nervous activity by traumatization, "collision between excitation and inhibition" (which often means conflict of motives), conditioned anxieties, and similar Pavlovian concepts.

A short book on the prevention of mental disorder has been written by Druzhinin (60). The booklet appears to be intended for the general public. Much of the book deals with the prevention of mental disease by avoidance of accidents, of alcohol and tobacco, by hygienic steps designed to prevent infectious disease, etc. The main points which might be of interest to psychologists are: The booklet incorporates the familiar ideas of Pavlov on nervous functioning as well as Makarenko's ideas on education. It follows the latter in putting stress on the educational value of participation in collective work and on the need to educate the child to love work. The mental hygiene value of maintaining a friendly family spirit and of parents taking the child seriously, lest the child develop lack of self-confidence, are mentioned. There are discussions of the desirability of cultural balance in education, and of the influence of appropriate literary models on personality development. There is also the claim generally made in the Russian psychiatric literature that the incidence of mental disorder in the Soviet Union is very low and is decreasing.

Books on psychological diagnosis and personality differences.—Three books dealing with psychological diagnosis and personality differences were examined. One of them, edited by Luria & Dulnev (61), deals with feeble-mindedness and its diagnosis. Medical techniques and the study of children's educational achievements, behavior in play, and conversational behavior are discussed. Some psychological techniques are presented which resemble subtests of certain American intelligence tests (e.g., picture arrangement), but their use is much freer and less standardized than in

American testing. There are also frequent references to experiments on "motor conditioning with verbal reinforcement," which are viewed as offering information about higher nervous activity in the Ss.

The book is not a report of research but is rather a textbook for workers on feeble-mindedness. It tends to be dogmatic. Feeble-mindedness is asserted to be based on brain disease, without any supporting evidence. The authors distinguish between feeble-mindedness and effects of educational neglect, without considering the possibility that the effects of the latter may be irreversible, or nearly so. The book contains general characterizations of the typical behavior of the feeble-minded in many situations. It does not indicate whether normal children ever also behave in these ways and tends to ignore changes of behavior with age. The diagnostic procedures are not standardized. The picture tests are described, but not reprinted; their source is not given. The authors suggest that diagnosticians may select their own pictures and cut them out of picture books. No tables of norms for different ages are presented. The discussions of the qualitative aspects of intellectual defect impressed this reviewer as often insightful. But the lack of a testing tradition in the Soviet Union appears to have resulted in the omission of most of the quantitative information which would be needed in order to evaluate (or use) the diagnostic procedures.

A book by Teplov and his associates (62) deals with experimental procedures for the determination of the Pavlovian types of nervous activity in people. The first paper in the book, by Teplov himself (63), presents a detailed case for his typological views, which depart from those of Pavlov in some respects (See 1, pp. 463, 469). Teplov's arguments are based in part on the experimental findings of his associates who contributed the other papers included in the book, and in part on an historical review of the development of Pavlov's views on types of nervous functioning. The review contains 265 references and, in a number of ways, is very instructive. It exhibits the essentially nonstatistical approach to individual differences which appears to be dominant in Russian scientific work; individual cases play the decisive role in the decisions about theoretical issues. It points out that Pavlov's own ideas on typology had undergone a number of major revisions, some of these having been undertaken when Pavlov found that one or two new experimental dogs did not fit into the previously accepted classificatory schemes.

The above mentioned paper shows that there are in Russia many disagreements about the doctrine of types, although some of the disagreements are obscured by the use of similar terms by different authors. Teplov argues that Pavlov's typology should not be viewed as final; Pavlov himself pointed out unsolved problems, and his many revisions of his "types," which continued until the last years of his life, emphasized this lack of finality. Teplov points out that there is only little correlational research on the consistency between different experimental indicators of the Pavlovian-postulated characteristics of nervous functioning, i.e., strength, mobility, and balance. He raises

a question about the validity of the classification of certain combinations of these characteristics as "main types" and others as "intermediate." He also points out that there is no reason to take it for granted that a person's type of nervous functioning should be necessarily the same for his different "analyzers," i.e., perceptual mechanisms.

It seems obvious to this reviewer that the last mentioned points made by Teplov can only be verified by large scale statistical research. However, there is no such research reported in the experimental papers in the book. There are some findings suggestive of high correlations, but they tend to be based on small numbers of cases and to involve experiments in which different aspects of the same or similar experimental techniques are compared to each other; thus Maisel (64) compared numbers of trials in extinction and differentiation experiments with a particular conditioned reflex in ten Ss. Good agreement between these (and some other) indicators of the inhibitory process is reported. In another study, by Nebylitsyn (65), certain indicators of the "strength" of processes in the visual system are compared in a number of subjects. One of these indicators has to do with the way visual thresholds are affected by extraneous stimuli elsewhere in the visual field. Another indicator involves the effect of caffeine on this reaction within the visual system. The two indicators gave concordant results. On the other hand, Ravich-Shcherbo (66) reported absence of correlations between a number of results of visual experiments which are in use in Russia as measures of "mobility of nervous processes." No correlations (or equivalent findings) between relatively dissimilar experimental procedures or between experimental results and observations of behavior are reported in any of the experimental papers in the book, so that the procedures described in them are based almost entirely on theoretical deductions; they lack statistical validation. This fact is one of the reasons these papers will not be reviewed in detail; another reason is the fact that much of the material in them had been presented by the authors and by Teplov earlier, and a portion of this material was summarized in last year's review.

Kovalev & Miasishchev's book on personality (67) is largely a theoretical and historical one. There is comparatively little original psychological material in it; this material consists of case histories of youth in which the combined effects of the family situation, the teachers, the "school collective," etc., on the developing individual are described by the authors. Characterizations of the political attitudes of the subjects are often included. The book also contains several guides for personality study, presented in the form of outlines. The theoretical and historical discussions of the book begin with Plato and Theophrastus. They deal with views of a number of French writers of the Seventeenth and Eighteenth Centuries, of Marx and Engels, of a number of Russian Nineteenth Century writers and educators, of Freud, Jung, and Adler, of contemporary Russian psychologists, etc. The historical discussions contain interesting information. The great importance of social relations in the personality make-up was em-

phasized by Marx and by a number of Russian writers of the Nineteenth Century. The general tone tends to be moralistic, and the book was written from the communist point of view; it is full of statements of disapproval of "typical bourgeois" personality, of "reactionary viewpoints" springing from the needs of the ruling classes in capitalist society, of approval of the "new Soviet man," etc.

Additional books.—Several additional books were examined. One of these books, by Luria & Yudovich (68), is a case study of identical, six-year-old twins, with severe speech defects. The apparent effects of the defective speech on the boys' intellectual functioning, behavior in play, constructive activity, etc., are described. The speech defects were viewed as caused in part by the mutual dependence of the twins on each other, so that they had only little need to communicate with others.¹⁷ The boys were placed in different groups in kindergarten. One of them received special instruction in speech; the other did not. The speech of both improved rapidly, particularly that of the boy who had received instruction. Changes in play behavior, intellectual functioning, etc., are discussed and attributed to the improved speech. The possibility that they might have been caused by the changed social situation is not mentioned in the book. The "improved speech" interpretation is defended in the book in terms of the congruence between the nature of the changes and this interpretation. The case of the twins is viewed by the authors as having theoretical importance as it provides an illustration of the role of speech in the complex interactive pattern of cognitive functioning.

Another book which was examined was the recently published collection of reprinted writings by Vygotsky, who died in the early 1930s (69). The collection contains an introduction by Luria & Leontiev. Vygotsky's writings are not recent enough to warrant reviewing in detail here. They are mainly a review of theories of cognitive functioning leading up to the author's theoretical interpretations. Some of author's own experiments are also included. The principal contributions of other authors which are discussed in the book include those of the Würzburg school, Lewin, Stern, the *Gestalt* school, and Piaget. The theoretical orientation of the book emphasizes the need to understand the exact nature of the relation between wholes and their parts which play a role in cognition; thus a considerable part of the book is devoted to the nature of the relation between sound and meaning as constituent parts of the whole, "meaningful word." Vygotsky's ideas constitute one of the important sets of antecedents to a number of attitudes which are prevalent in modern Russian psychological theories.

A book by Makarov, entitled *Neurodynamics of Man* (70) deals mainly with the effects of direct stimulation of interoceptors in the stomach, colon, rectum, and urinary tract of human subjects. Chemical and electric

¹⁷ It is also mentioned by the authors that the boys' mother and her brother both had had severe speech defects in childhood.

cal stimuli were used. The responses which were studied included verbal report, local motor reactions, GSRs, EEGs. There were great differences in sensitivity, as revealed by these indicators. Verbal report tended to be the least sensitive of the indicators. Direct stimulation produced GSRs much before the subjects reported any sensation. These GSRs to stimuli which the subject could not report are described as sometimes more intense than those which occur in response to painful electrical stimulation of the skin.

Periodicals.—Because of limitations of space, it is not possible to review more than a very small number of papers which appeared in periodicals. The papers have been chosen mainly on the basis of presence of features which have not been previously noticed in Soviet psychology.

Kedrov's paper (71) deals with the process of scientific discovery, using mainly Mendeleyev's development of the periodic table of chemical elements as an example. The author points out that the course of this discovery could be reconstructed in great detail because of the availability of a large number of documents. The discovery was made in one day. The first relevant crude notes by Mendeleyev were found to have been made by him on the back of a letter which he had received that morning and apparently read during breakfast (as shown by the mark made by a drinking cup which had been placed on the letter). At the end of the day a form of the periodic table was ready for publication. The discovery was prepared by the scientist's earlier work, but it is pointed out that he had not anticipated making it and had not expected to engage in serious scientific work. He had planned to go on a trip which was motivated by his interest in the cooperative movement. The discovery of the periodic table began as a by-product of his thinking about a practical decision which he had to make; he had just finished writing a section of a book on chemistry, and had to decide what chemical elements to discuss next. This problem required consideration of relations between elements, and the notes on the back of the letter indicate that in dealing with these relations the scientist began to arrange groups of chemical elements in order of their atomic weights. Further exploration indicated relations between atomic weight and other properties; this finding enabled Mendeleyev to make the practical decision about the order of chapters in his book. By that time, however, it must have been apparent to him that he was about to discover an important general principle of chemistry. The subsequent work involved a great deal of arranging and rearranging of cards with symbols of chemical elements written on them. The author believes that Mendeleyev hit upon this scheme for three reasons: he worked against time (he had committed himself to go on the trip); he is known to have liked to play solitaire; and he thought highly of another chemist who had made similar use of cards in other chemical problems.

A paper by Levitov (72) is a review of the work of Alfred Binet and commemorates the hundredth anniversary of his birth. The attitude ex-

pressed by the author towards Binet's work is generally favorable, in spite of disapproval of his philosophical idealism and his scale of mental development. The latter is criticized only briefly, apparently on the basis of its inadequate theoretical foundations. It is characterized as far superior to the American test scales which developed out of it and which are criticized as using techniques which are not sufficiently individualized. Binet is quoted as advocating careful study of individuals rather than mass testing. His basic idea of finding out how intellectual functioning develops with age is characterized as sound. Most of the paper is devoted to Binet's achievements in fields other than the determination of mental age. Study of Binet's work is recommended. It is worth noting that the bibliography includes titles of Russian translations of six books by Binet which appeared in Russia between 1889 and 1911.

A theoretical paper by Bernstein (73) deals with the nature of regulation of movement by the individual and attempts to treat the system which accomplishes this as a servomechanism. The paper is too complicated for a brief review, but it should be noted that some Pavlovian ideas are explicitly criticized in it. Thus the author argues that some conditioned reflexes are based on sensory-motor associations rather than on associations within the sensory system. He raises objections against the characterization of speech as the second signal system, stating that the function of speech as a signal to which the organism responds by action is only one of its more primitive functions; even animals may learn to obey simple orders. The truly human function of speech is to function as a system which reflects the external world and the individual's interaction with it. This paper, published as dealing with a controversial topic (according to an editorial footnote), expresses the least favorable attitudes towards Pavlov's ideas which this reviewer has seen in modern Russian scientific writings.

A new paper by Teplov (74) contains evidence of further development of his typological ideas beyond the level of the book edited by him and discussed earlier in this paper. Teplov explicitly points out that a number of Pavlovian and post-Pavlovian typologies have little in common with each other except for an unfounded acceptance of "four" as the number of temperaments. He argues that Pavlov's essential achievement had to do with his suggested typological dimensions (strength, mobility, and balance), which probably can be combined in many ways. The author calls for research, and apparently some of it will be correlational in type.

A paper by his associate Nebylitsyn (75) does contain a correlation; the correlation is one between two measurements of "strength" of nervous functioning in the case of vision and audition; the familiar rank difference formula is used. A low correlation (.26) was obtained, which the author characterizes as indicative of the absence of an appreciable correlation; but he also rather inconsistently states that there was substantial agreement within the measurements in 72 per cent of the cases, not defin-

ing "agreement" or examining the question of what percentage of agreement should be expected in the case of a zero correlation.

A correlation-like finding is also reported in a paper by Vitenson (76) who presents the relationship between two variables in the form of a contingency table. One of these variables was the Pavlovian type, determined from a large number of observations of subjects in nonexperimental situations; the criteria are named but are not described in detail. The other variable had to do with latency and duration of visual afterimages under various conditions (drugs, oxygen deprivation, sleep deprivation, etc.). The contingency table indicates a high correlation between the variables, but the author does not specifically mention whether an essential precaution was observed, i.e., it is not clear whether the determination of the Pavlovian type was made without knowledge of the experimental results with afterimages.

Volkova (77) reports on conditioning experiments with early adolescents in which initially the word "correct" was used as the conditioned stimulus. It was repeated six times after which the subject was offered a berry which he was allowed to take in his hand and eat. Both the grasping reaction and the salivation were recorded. The word "error" was also used in the experiments, but was not followed by an offer of a berry. Thus the subjects acquired "conditioned motor and secretory reflexes" to the word "correct," but not to "error." Thereupon true and false statements were substituted for "correct" and "error" and were found to function like these words. Once this stage was reached, it was found that the same statement could function either to produce the conditioned reflexes or not to produce them, if circumstances changed the statement from true to false. Thus "Today is January 2." and "Today is January 3." exchanged their functions as "conditioned" and "differentiation" stimuli when tried out on these two days. The author comments about the influence of changes of the real situation on the functioning of conditioned reflexes.

Usievich (78) reviews a number of studies which deal with alterations of the condition of the blood of experimental animals by factors which "overstrain" excitatory and inhibitory processes in the central nervous system. These factors included, e.g., the use of excitatory and inhibitory drugs, experiments requiring very fine differentiation, changes in intensity of the conditioned stimuli and the differentiation stimuli, etc. Changes in the rate of production of blood particles and in the sugar content of blood, etc., are reported. The author discusses the results in relation to Selye's theory of the general adaptation syndrome; he criticizes this theory for its neglect of cortical control of adrenal and pituitary functioning.

Belenkov (79) reports the results of almost total decortication of cats on what the author calls their complex unconditioned reflexes. Some of his findings were: decorticated cats tend to respond to the presence of food with great excitement (if hungry); if their olfactory lobes are preserved, the cats are likely to find the food fairly promptly; if the olfactory lobes

are destroyed, the animals' movements cease to be directed toward the food. The animals' reactions are reported to have been particularly violent whenever a mouse (live or recently killed) was presented. Sexual behavior of decorticated cats was also observed. Although decortication causes sexual behavior to become disorganized, it apparently does not abolish the sex drive, nor in all instances prevent sexual performance. One of the author's decorticated male cats, after being placed in a small cage with a (half-decorticated) female, impregnated her.

Zverev (80) distinguishes between two kinds of experimental neuroses in dogs. Dogs having one kind of neurosis continue to accept food in the experimental situation while those having another kind refuse it. The author suggests that transmarginal inhibition affects the cortex in the first kind of neurosis, spreads to subcortical centers in the second kind. He also reports that neuroses of the first kind are readily cured by sleep of long duration; those of the second kind are resistant to sleep therapy.

Malinovsky (81) reports on results of his experiments on instrumental conditioning, in which the amount of physical work accomplished by the animal in pulling a ring was measured in energy units. If the animal pulled more than once in response to one conditioned stimulus, the total amount of work accomplished was determined by addition of the separate energy measures. The subjects were rabbits and food was used as the incentive. It was found that the amount of work done by the animals tended to increase if the conditioned stimulus was intensified. However, further intensification of the stimulus sometimes resulted in reduction of the energy (transmarginal inhibition). The expenditure of energy was also found to be a function of changes in the "basic ration" fed to the animals. When the basic ration was increased, the expenditure of energy decreased temporarily. The expenditure of energy tended to return to its initial level as the animal became adjusted to the increased amount of food (as shown by its ceasing to gain weight). Similarly, a decreased food ration tended to result in a transitory increase in expenditure of energy. More permanent changes in the expenditure of energy resulted if the increases in the food ration were made in several small steps.

Roginsky (82) investigated the sensory control of the white rat in the maze. Both enclosed and open mazes were used. The author suggests that maze running is guided by multiple sensory cues but that kinesthesia is likely to play a leading role, particularly after considerable practice. The facts cited in favor of the leading role of kinesthesia in maze running are: when rats are blindfolded, their maze running is not seriously disturbed; when short-cuts are introduced, rats ignore them (the author does not state for how many trials). The chief arguments in favor of multiple sensory control are: when the visual situation is changed (e.g., by adding an electric light to mark the correct path), the rats explore the situation visually and by sniffing; if such a distinctive sign is then moved to a different alley, where a new true path is opened, the rat follows the sign. If the maze is

moved to a different part of the room, the maze performance is disturbed. In discussing theories of sensory control in the maze, the author exaggerates the acceptance of Watson's old kinesthesia theory in American psychology.

Chailakhian (83) attempted to obtain conditioned contraction responses in hydras. The experiments seemed to lead to success, but the author notes that the "conditioned" contractions only took place if the time interval between the last unconditioned and the supposedly conditioned stimulus did not exceed a few minutes. The author suggests that the seemingly positive results were the effects of stimulus summation in the slowly functioning nervous system of the hydra, rather than true conditioning.

A paper by Volokhov (84), who is Vice-Editor-in-Chief of the *Journal of Higher Nervous Activity*, is a eulogy of the work of Orbeli, written on the occasion of his seventy-fifth birthday. It was mentioned in (1) that Orbeli was much criticized at the joint meeting of two scientific academies in Moscow in 1950. Volokhov's paper is highly complimentary. It reviews Orbeli's long career of distinguished scientific achievement, which does not seem to be known to psychologists in this country. Orbeli is reported to have obtained negative results in attempting to localize the cortical mechanisms of conditioned reflexes by surgical removal of certain so-called association areas of the cortex in animals. This work, which appears to have points in common with that of Lashley, was done by Orbeli before World War I.

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